Contextual and Temporal Influences on Abstract Cognitive Control

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"If I pick up my cigar, I do not will to move any specific muscles. Indeed in many cases, I do not know what those muscles are. What I do is to turn into action a certain feedback mechanism; namely, a reflex in which the amount by which I have yet failed to pick up the cigar is turned into a new and increased order to the lagging muscles, whichever they may be. In this way, a fairly uniform voluntary command will enable the same task to be performed from widely varying initial positions, and irrespective of the decrease of contraction due to fatigue of the muscles."

Norbert Wiener

"The central nervous system is complicated, and therefore its attributes and characteristics have every right to be complicated. Let not our facile familiarity with it, through the medium of the subjective consciousness, fool us into illusions in this respect."

John von Neumann in a letter to N. Wiener

Acknowledgements

Contents

List of figures

List of tables

List of abbreviations

ACC	**A**nterior **C**ingulate **C**ortex
BF	**B**ayes **F**actor
BRAC	**B**inding and **R**etrieval in **A**ction Control
CE	**C**ongruency **E**ffect
CSE	**C**ongruency **S**equence **E**ffect
C-CSE	**C**ontext-specific **C**ongruency **S**equence **E**ffect
ITI	**I**nter-**T**rial **I**nterval
OSF	**O**pen **S**cience **F**ramework
PFC	**P**refrontal **C**ortex
PCE	**P**roportion **C**ongruency **E**ffect
R	**R**esponse
RSI	**R**esponse-**S**timulus **I**nterval
RT	**R**eaction **T**ime
S	**S**timulus
SC	**S**witch **C**osts
SD	**S**tandard **D**eviation
TEC	**T**heory of **E**vent **C**oding

List of publications

This is a cumulative dissertation that includes two published papers and two manuscripts that are submitted for publication.

Author contributions are declared according to § 5 Abs. 2 No. 8 of the PhD regulations of the Faculty of Science

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Contributions by the candidate:

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Summary

Goal-oriented behavior requires the organization and coordination of subordinate processes, particularly when multiple processes compete for the same resources. The cognitive mechanisms enabling this coordination are referred to as cognitive control. These mechanisms encompass the allocation of attentional weights to specific facets of a task and the implementation of rules crucial for task performance. Furthermore, they can operate at various levels, from specific stimulus and action codes to more abstract features such as relational information. Notably, the transfer of states of cognitive control from one episode to the next is impaired if they do not occur within the same context. Episodic binding and retrieval frameworks posit that this context specificity of control states reflects binding between the control states and the cooccurring context, where the context subsequently serves as a retrieval cue for the bound control state.

This dissertation examines three facets of abstract cognitive control. First, two studies investigated the temporal stability of abstract control states and the bindings between such control states and visual contexts. The findings indicate that abstract control states and their bindings are invariant to temporal delays of several seconds. The discussion explores the extent to which the temporal dynamics of control and bindings can be attributed to the level of abstraction of the bound features. Second, one study examines the control retrieval process, as proposed by theories of episodic binding and retrieval, to scrutinize its role as the source of effects of contextualized control. Three experiments provide decisive evidence against such a retrieval process. Instead, contextualized control appears to be more effectively explained by the disruption of maintained control states. In the discussion, I connect these findings to theories suggesting that contextual features control the updating of working memory content. The final study generalizes the insights gained from studies on contextualized attentional weighting to task control by demonstrating that abstract task rules can also be bound to visual contexts. I discuss an approach for integrating bindings acting on different levels of abstraction into a unified model of task control.

Zusammenfassung

Zielgerichtetes Verhalten erfordert die Koordination untergeordneter Prozesse, insbesondere wenn diese um die gleichen Ressourcen konkurrieren. Diese Mechanismen werden als kognitive Kontrolle bezeichnet und umfassen beispielsweise das Lenken der Aufmerksamkeit auf bestimmte Aspekte einer Aufgabe sowie die Implementierung von Regeln, die für die erfolgreiche Aufgabenerfüllung entscheidend sind. Von abstrakter kognitiver Kontrolle kann gesprochen werden, wenn Koordinationsprozesse nicht anhand spezifischer Reize oder Handlungsabläufe implementiert werden, sondern auf abstrakten relationalen Zusammenhängen beruhen. Kognitive Kontrollzustände können schlechter von einer Episode zur nächsten übertragen werden, wenn diese nicht im selben Kontext stattfinden. Theorien für episodisches Binden und Abrufen postulieren, dass diese Kontextspezifität von Kontrollzuständen auf Bindungen zwischen den Kontrollzuständen und dem gleichzeitig auftretenden Kontext zurückzuführen ist, wobei der Kontext anschließend als Abrufhinweis für den gebundenen Kontrollzustand dient.

Diese Dissertation geht auf drei Facetten abstrakter kognitiver Kontrolle ein. Erstens, untersuche ich in zwei Studien die zeitliche Stabilität abstrakter Kontrollzustände und ihrer die Bindungen mit visuellen Kontexten. Es zeigt sich, dass abstrakte Kontrollzustände und ihre Bindungen stabil über zeitliche Verzögerungen von mehreren Sekunden sind. Inwieweit die zeitliche Dynamik von Kontrolle und Bindungen auf das Abstraktionsniveau der gebundenen Merkmale zurückgeführt werden kann wird diskutiert. Zweitens untersucht eine Studie den Prozess des Kontrollabrufs, wie er von Theorien des episodischen Bindens und Abrufens vorgeschlagen wird, um seine Rolle als Ursprung kontextualisierter Kontrolleffekte zu überprüfen. Drei Experimente erbringen entscheidende Evidenz gegen einen solchen Abrufprozess. Stattdessen kann kontextualisierte Kontrolle effektiver durch die Störung aufrechterhaltener Kontrollzustände erklärt werden. Die vierte Studie verallgemeinert die Erkenntnisse aus Studien zur kontextualisierten Aufmerksamkeitsgewichtung auf die Aufgabenkontrolle, indem sie zeigt, dass abstrakte Aufgabenregeln ebenfalls an visuelle Kontexte gebunden werden können. Ich diskutiere einen Ansatz, wie Bindungen, die auf unterschiedlichen Abstraktionsebenen wirken, in ein einheitliches Modell der Aufgabenkontrolle integriert werden können.

Chapter 1

Introduction

A unique characteristic of human cognitive capabilities is the range and flexibility of complex behavior. We perform actions to achieve our goals based on information that our cognitive system detects to be meaningful to achieve our action goals. We possess the ability to stay focused on our current objectives for prolonged durations and shield them against the influences of distracting information. In challenging situations, we can derive the correct actions according to complex rules. The term *cognitive control* refers to the collection of cognitive processes that orchestrate various subordinate brain processes in accordance with situational demands (Cohen, 2017; Diamond, 2013; Gilbert & Burgess, 2008; Miller, 2000). Such control processes can act on different levels. Sometimes it is sufficient to control the selection of specific stimuli and actions. For example, a red traffic light always requires breaking. But the world is often complex and we must generalize beyond such direct stimulus-to-action mappings. In such cases cognitive control can be labelled as *abstract.*

Often, environmental variables determine what is relevant to solve a challenging situation successfully. Consequently, such context variables are central to the implementation of cognitive control (Bugg, 2017; Chiew & Braver, 2017; Egner, 2023). Most of the work has focused on the role of context that is contingent with certain task demands, and therefore can instruct how control is to be adjusted to address these challenges (Crump et al., 2006; Crump & Logan, 2010). However, also contextual variations that do not indicate specific task demands, were shown to have an influence on cognitive control (Dignath et al., 2019; Spapé & Hommel, 2008). While traditional learning models have difficulties to explain these effects of incidental context-to-control pairings (Abrahamse et al., 2016; Blais et al., 2007), episodic binding and retrieval theories provide a parsimonious explanation (Frings et al., 2020; Hommel et al., 2001). These theories would describe such effects as automatically occurring bindings between contexts and the control states and thus making control states specific to the bound context.

In the following, I will review literature on cognitive control and its interplay with different kinds of context, before exploring how episodic binding and retrieval can provide an underlying model for context effects on cognitive control.

1.1 Cognitive control

The current concepts of cognitive control have their roots in early theoretical work that proposed that certain cognitive processes are inherently volitional and controlled, whereas other processes are automatic and involuntary (e.g., Neumann, 1984; Shiffrin & Schneider, 1977). Posner and Snyder (1975) illustrated this through a variant of the *Stroop task* (MacLeod, 1991; Stroop, 1935). Here, participants were presented with color words that were presented in a colorized font and were instructed to identify either the word or its font color. They posited two key reasons for categorizing word reading as an automatic process and color naming as a controlled process. First, word reading is executed faster than color naming, aligning with the conventional notion that automatic processes are faster than controlled processes. Second, the semantic meaning of the word heavily influenced the performance in naming the font color but not vice versa, indicating that the controlled and consequently voluntary color naming process is affected through automatically processed conflicting information from word reading. Subsequent research, however, challenged this assumption of process dichotomy. Depending on the task demands and individual learning experiences, processes previously regarded as automatic could also be influenced by what was previously described as controlled processes (MacLeod & Dunbar, 1988). Consequently, modern computational theories of cognitive control renounce from an assumption of dichotomous processes but instead describe information processing on a continuum of automaticity (e.g., Cohen et al., 1990; Cohen et al., 1992; Feng et al., 2014; Musslick et al., 2016).

In influential connectionist control models, a state of cognitive control is defined as a set of parameters controlling the activation flow along a specific pathway, such as the connection from color naming to responding with the corresponding color key in the Stroop task. Such a precedented activation flow within a pathway allows its units to activate ahead of competing pathways sharing local modules. As units attain higher activation levels, they become less susceptible to inputs from other, later-activated pathways. Consequently, the parameters that control the activation flow of pathways also control how competition for local units is resolved (Cohen et al., 1992). In other words, from a connectionist perspective, one can envision the brain as a conglomerate of pathways that connect sensory input to behavioral output. Depending on the current control state, specific pathways can be amplified to prevent crosstalk

with other competing pathways (for alternative Bayesian or reinforcement learning models of cognitive control, see for instance, Brown & Braver, 2005; Holroyd & Coles, 2002).

Cognitive control theories assert that the functioning of control processes relies on activity within the *prefrontal cortex* (PFC; Friedman & Robbins, 2022; Koechlin et al., 2003; McGuire & Botvinick, 2010; Miller, 2000; Ridderinkhof et al., 2004). Importantly, this does not imply that every neural pathway in the brain must pass through the PFC. Rather, the activity within the PFC exerts control over the flow of activity in pathways situated across other cortical and subcortical regions (Botvinick & Cohen, 2014; Brass et al., 2005; Cohen et al., 1994; MacDonald et al., 2000). Consequently, depending on the activity pattern within the PFC, selected subordinate pathways are favored to cause behavioral output. Following this logic, the state of the PFC controls the production of meaningful, goal-directed behavior. Therefore, it can be conceptualized as representing action goals, including all necessary components such as attentional weights, task rules, declarative information, etc. (Miller, 2000).

A central attribute of cognitive control must be its flexibility (e.g., Rougier et al., 2005). Situational demands and task goals may not only differ in the duration for which they are expected to be relevant but also change dynamically. This poses several challenges to the cognitive system. Under which circumstances should the currently active control representation be updated, which control representations are appropriate, and for how long should they be held active? In the upcoming sections, I will review the body of literature that addresses these critical questions, focusing on the topics of attentional weighting, conflict adaptation, task control, and temporal continuity of control states.

1.1.1   Attentional weighting

A fundamental role of cognitive control is to flexibly adapt attention, enabling the selective prioritization of task-relevant information over task-irrelevant information (Botvinick et al., 2001; Gratton et al., 1992). Since this prioritization of different information sources is always described in mutual relation, one can speak of attentional weights (e.g., Liesefeld et al., 2019). For an experimental test of such attentional weights, researchers commonly use response interference tasks such as the *Stroop task*, the *flanker task* (B. A. Eriksen & Eriksen, 1974; C. W. Eriksen, 1995), the *Simon task* (Simon, 1990), or more recently, the
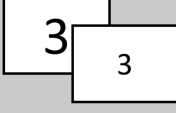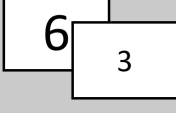
	Stroop	Simon	Flanker	Prime-Target
Congruent	RED	←	SSSSS	3 / 3
Incongruent	BLUE	←	HHSHH	6 / 3

FIGURE 1: This figure displays congruent and incongruent trials for the widely used Stroop task, Simon task, flanker task and prime-target task. All tasks encompass a task-relevant dimension that instructs the required response (Stroop: the font color in which a color word is displayed; flanker: the central symbol of multiple symbols; Simon: the color/shape of the stimulus displayed at different locations; prime-target: the second of two consecutive stimuli) and a distractor dimension that is helpful in congruent trials but distracting in incongruent trials (Stroop: the semantic meaning of the word; Flanker: the flanking symbols; Simon: the location of the stimulus; prime-target flanker: the first stimulus).

prime-target task (Hazeltine et al., 2011). These tasks share a common structure (see Fig. 1): Participants are instructed to react to a task-relevant target dimension but to ignore the information provided by the distractor dimension. Importantly, the distractor can be helpful by inducing the same response tendency as the target (so-called congruent trials). Other times, distractors can be distracting in that they induce a response tendency that differs from that of correct response (incongruent trials). In all these tasks, *congruency effects* (CEs) can be robustly observed as the difference in performance between congruent and incongruent trials, where performance is often measured in reaction times: $CE = RT_{inc} - RT_{con}$. CEs can be interpreted as a relative measure of the attentional weights that are distributed between the target and distractor dimensions. An absence of the CE ($CE = 0$) indicates that no attention is allocated toward the distractor dimension since its information does not affect task performance. Conversely, larger CEs indicate that more attentional weight is allocated toward the distractor dimension. Since CEs can be flexibly modulated by a variety of task properties (for example, see Bugg, 2017; Egner, 2007; MacLeod & Dunbar, 1988), they are widely used as a measure of implemented control states (Cohen et al., 1990).

1.1.2 Conflict adaptation

Interestingly, trial congruency affects not only behavior in the current trial but also in subsequent, trials indicating that control states are dynamically adjusted depending on the level of congruency previously experienced. These effects manifest both on a trial-by-trial basis, commonly known as the *Congruency Sequence Effect* (CSE), as well as over a series of trials, often referred to as the *Proportion Congruency Effect* (PCE). The CSE, initially described by Gratton et al. (1992), captures the phenomenon that CEs are modulated by the congruency level in the preceding trial: After incongruent trials, CEs tend to be smaller than after congruent trials. The CSE is calculated as follows: $CSE = (RT_{inc} - RT_{con})_{(preceding\ congruent)} - (RT_{inc}RT_{con})_{(preceding\ incongruent)}$. Similarly, the PCE refers to reduced CEs after participants were exposed to a list of mostly incongruent trials in comparison to CEs after mostly congruent trials (Logan & Zbrodoff, 1979).

These observations of adaptive CEs have sparked extensive debates about the underlying mechanisms. Early work reasoned that participants strategically adapt control states to the experience of congruency (Gratton et al., 1992; Logan & Zbrodoff, 1979). However, this perspective came under later criticism since it lacked a clear formalization of a mechanism that detects the need for control adjustments. Instead, a strategic, volitional component in control adaptation was assumed. Such theoretical gaps, relying on an intelligent and controlling agent to fill them, are referred to as *homunculus problems*, a metaphorical allusion to a small human-like figure "who does all the marvelous things that need to be done actually to generate the total behavior of the subject" (Newell, 1980, p. 716; see also Monsell & Driver, 2000; Verbruggen et al., 2014).

To overcome the homunculus problem in control adaptations, Botvinick et al. (2001) introduced the *conflict monitoring theory*. This seminal computational approach to control adaptations comprises a recursive loop of conflict monitoring and control adaptations. As previously illustrated, the primary objective of cognitive control is to minimize interference between pathways. In the Conflict Monitoring Theory, the authors assumed an additional system that monitors for such conflicts in local modules (black module in Fig. 2). To provide a more concrete example, if a task involves two possible responses activating both response units during the response selection process (e.g., one by the target and one by the distractor information), such a conflict would be detected by

the monitoring system. While experimental operationalizations of conflict often revolve around response conflict (Barch et al., 2001; Botvinick et al., 2001), conflict can manifest at various processing levels, including stimulus discrimination (Milham et al., 2003; van Veen & Carter, 2002; Weissman et al., 2003) and task representations (Badre & Wagner, 2004). On a neurophysiological level, the conflict monitoring system is believed to reside in the dorsal anterior cingulate cortex (dACC), from where it signals to the PFC that control adjustments are necessary to resolve conflicts in the system (Botvinick et al., 2004; Kerns et al., 2004; van Veen & Carter, 2002). To compute the strength of conflicts in the system, the conflict monitoring theory makes use of a simplified version of the *Hopfield energy* in neural networks: $E = -\sum_{i,j} a_i a_j w_{ij}$ with a indicating unit activity, the subscripts i,j indicating the units of the network, and w_{ij} indicating the weight of the connection from unit i to j (Hopfield, 1982). In the simulation studies of Botvinick et al. (2001), conflict was modeled as Hopfield energy but only in the response layer with two mutually inhibiting response units. Thus, the energy-reducing effect of simultaneous activation of excitatory connected units, as described in the original version (Hopfield, 1982), was not implemented. Furthermore, this means that the simulations only operationalized monitoring for response conflict. Here, no conflict exists if none or only one response unit is active. When both response units are concurrently active, response conflict arises as a function of the response unit's activation. The conversion of the detected conflict E into a new control state C, representing the new activation strength of the module responsible for control implementation, is described by the formula $C(t+1) = \lambda C(t) + (1 -)(\alpha E(t) + \beta)$ where t indicates the trial; α and β are scaling parameters weighting the influence of most recent conflict; λ relatively weights the experienced conflict to the current control value, akin to the learning rate parameter in reinforcement learning (Barto, 1997). In each trial, control is adjusted either up or down based on the response conflict in previous trials. Since control adaptations are grounded in the cumulative outcomes of all prior conflict experiences, this mechanism can account for sustained control adjustments resulting from task demands over an extended timeframe. This computational approach conceptualizes the CSE as the consequence of sequential conflict adaptation and the PCE as a result of accumulated conflict adaptation over the course of several trials.

However, alternative explanations have been put forward to challenge the notion of conflict-driven adjustments in cognitive control as the origin for the CSE and the PCE. Episodic binding and retrieval accounts posit that in task designs

in which stimuli and responses can repeat across trials, differences in retrieval conditions can produce a pattern resembling the CSE (Hommel et al., 2004; for a detailed explanation see section *Binding and Retrieval in Experimental Research*). Often, such accounts cannot be distinguished from conflict-triggered control adaptations because the critical trial sequences of both accounts are identical. Further, the PCE (and certain paradigms with the aim of measuring CSEs) has been challenged by alternative explanations through the learning of contingencies between the distractors and the required responses (Schmidt & Besner, 2008; Schmidt & Houwer, 2011). For lists with mostly congruent trials, participants may learn that, in most cases, distractor words indicate the correct response. Conversely, for lists predominantly comprising incongruent trials, they may learn that the required response often differs from the one suggested by the distractor (Schmidt et al., 2007). This effect is particularly pronounced in paradigms with only two response keys, a common setup in psychological research, as the distractor can be directly associated with the mostly correct response (i.e., the one suggested by the distractor or the respective other one) depending on whether the current list is predominantly congruent or predominantly incongruent (Schmidt et al., 2007).

The debate over whether control-related phenomena in response interference tasks are better explained by associative processes between stimuli and responses or adaptations in control states weighting attention between different aspects of the present task has engaged the field for almost a decade (see e.g., Duthoo et al., 2014b; Egner, 2007, 2014, 2017; Schmidt, 2013; Weissman et al., 2014). Reconciling these perspectives, recent theoretical (Egner, 2014) and empirical work (Jiang et al., 2015) suggests that control mechanisms can act on different levels of *abstraction*. Here, I will distinguish between nonabstract and abstract control. Nonabstract control refers to cognitive control that primarily hinges on the strength of associations between perceptible features, such as specific stimuli or responses. However, abstract cognitive control relies on the integration of relational information or complex task rules that necessitate the incorporation of additional contextual information.

To measure abstract control states, it was proposed to analyze CSEs and PCEs in specific trial sequences: When stimuli and responses cannot repeat between the control-inducing trials (i.e., trials in which conflict can be induced) and the control probing trials (i.e., the trials where CEs are measured) and

when each specific response of the control probing trials is equally often assigned with each level of congruency, neither the retrieval of stimulus or response features nor contingency learning can account for observed CSEs or PCEs. Consequently, the control adaptation effects observed in such designs bolster the perspective that conflict-driven adaptations in attentional weights are at play. Such trial sequences can be achieved if simple rules are implemented. A set of at least four stimuli with matching responses is divided into two subsets, A and B. Importantly, A does not contain any stimulus or response features that are prevalent in B or vice versa (Braem et al., 2019). To measure CSEs, in trials with odd indices, target and distractor stimuli are selected from one subset, while the respective other subset is assigned to trials with even indices. This results in ABAB[…] subset sequences, a design some refer to as *confound-minimized* (Gyurkovics et al., 2020; Jiménez & Méndez, 2013; S. Kim & Cho, 2014; Weissman et al., 2014). To investigate PCEs, so-called *transfer* or *inducer-diagnostic* designs have been proposed (Weidler & Bugg, 2016). In this approach, a list of stimulus-response pairs from one subset, along with a manipulation of proportion congruency, induces a control state. This control state is subsequently probed with a list of stimulus-response pairs from the other subset, in which no proportion congruency manipulation exists, that is, congruent and incongruent trials are equally likely to occur (Bugg, 2014; Bugg & Chanani, 2011; Hutchison, 2011; Wühr et al., 2015).

1.1.3 Task control

The preceding sections focused on the interplay between distracting and helpful information and the mechanisms that allow the cognitive system to adapt to control states appropriate for experienced task demands. While the guidance of attention is undoubtedly a pivotal function of the PFC, there exists a broader spectrum of control functions essential for a comprehensive understanding of goal-directed behavior. A perspective that aligns more closely with human everyday behavior emerges from the field of task control research and is concerned with the processes required to execute a task and to switch from one task to another (Kiesel et al., 2010; Koch, Poljac, et al., 2018).

One of the central paradigms in this research area is task switching. In such experiments, participants are successively trained to perform two or more distinct tasks. These tasks can encompass a range of activities, from discriminating stimuli based on certain criteria, such as determining if a displayed number is
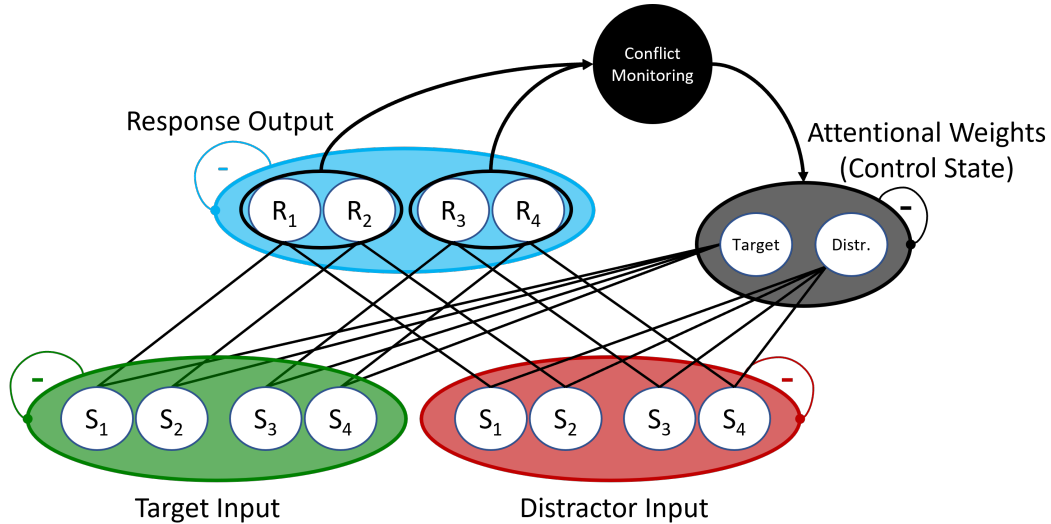
FIGURE 2: Structure of the loop between conflict monitoring and control implementation for response interference tasks in which stimulus and response sets alternate across trials (set a: S_1R_1, S_2R_2; set b: S_3R_3, S_4R_4; based on Botvinick et al., 2001; Cohen et al., 1992). The response output layer (blue) receives excitation from the target input layer (green) and the distractor input layer (red). The attentional weights layer (gray) controls the activation of the input layers. The conflict monitoring system (black) receives information about activation in the output layer and adjusts the attentional weights accordingly. A control state is defined as a specific pair of attentional weights.

even or odd (Allport et al., 1994; Rogers & Monsell, 1995), to executing arithmetic operations (Baddeley et al., 2001; Rubinstein et al., 2001) or performing spatial stimulus operations (U. Mayr & Kliegl, 2003). Following this task training, participants then engage in experimental blocks during which the tasks alternate in predictable (Rogers & Monsell, 1995) or unpredictable order (Meiran, 1996). Typically, performance in trials where the task repeats from the previous trial is better than in trials where a new task must be performed, a phenomenon commonly referred to as *switch costs* ($SC = RT_{\text{task switch}} - RT_{\text{task repetition}}$); e.g. G. Wylie & Allport, 2000). SCs are commonly interpreted as indicator of participants adopting specific task sets to shield and organize multiple individual tasks (Rogers & Monsell, 1995). A task set refers to the collection of mental representations that guide the identification of task-relevant stimuli, the selection of an appropriate response, and response execution (Schneider & Logan, 2014; Vandierendonck et al., 2010). Empirical research has shown that SC cannot be attributed solely to the overlap of specific stimulus-to-response mappings. These findings underscore the necessity of higher-order control processes that translate stimulus input into motor output based on abstract task rules (Dreisbach et al., 2007; Haynes et al., 2007; Kikumoto & Mayr, 2020; U. Mayr &

Bryck, 2005; Waszak et al., 2003; for a review, see Hazeltine & Schumacher, 2016). Consequently, when the tasks are controlled for stimulus and response overlap across trials (see Jost et al., 2013; Schmidt & Liefooghe, 2016; Schmidt et al., 2020), SCs must be a correlate of abstract task control processes. It was suggested that these could reflect interference stemming from previously implemented but now interfering representations of task rules governing the stimulus-appropriate response selection or costs of processes that are required to update these representations (Allport et al., 1994; Rogers & Monsell, 1995; Rubinstein et al., 2001).

1.1.4 Temporal continuity of cognitive control

Although determining the duration for which control states are maintained is acknowledged as an important question in the literature (Braver, 2012; Cohen, 2017; Egner, 2023), no conclusive answer can be deduced from the previously described control models. The conflict monitoring theory predicts updates in the implemented control state as a function of trials and the level of conflict that is induced by those trials but does not establish a direct link to elapsed time between the implementation and measurement of control states (Botvinick et al., 2001). Braver (2012) proposed that to achieve the required flexibility of the human cognitive system, two distinct modes of cognitive control are required, each characterized by qualitatively different temporal dynamics. The author contemplated that *proactive control*, which is implemented in anticipation of upcoming conflict, must be maintained over time, and *reactive control*, a stimulus-driven, just-in-time mechanism to overcome conflict, is of transient nature.

In experimental research, two studies have reported that the CSE diminishes when an interval of approximately 3 to 5 seconds is introduced between the control-inducing and control-probing trials (Duthoo et al., 2014a; Egner et al., 2010). The system's flexibility is underscored by the finding that the CSE appeared to be temporally invariant when participants were strictly incentivized to adopt proactive control strategies by incorporating only extended intervals between trials (Duthoo et al., 2014a). However, these studies were constrained by the use of paradigms in which stimuli and responses could be repeated across trials. Hence, control processes implemented within such task designs might conceivably rely on the weighting of associations between specific stimulus and response combinations (Hommel et al., 2004; see section 1.3.1). It seems reasonable to assume that more action goals requiring abstract cognitive

control are often temporally distant because by definition they are not specified for the current situation. On the contrary, control states established based on specific stimulus or response codes must be implemented in the presence of a particular feature constellation, thereby suggesting that the targeted action goals are likely to be more immediate. Following this logic, it seems reasonable to assume that the abstraction level on which control representations operate influences their time course (Badre, 2008).

1.1.5   Interim summary

In the preceding sections, I have defined cognitive control as a collection of cognitive functions that control the activation flow of subordinate processes competing for shared neural units. A control state corresponds to a specific set of parameters of these functions. Moreover, the conflict monitoring account posits that cognitive control needs to be adjusted when the system detects conflict in neural units. In response interference paradigms, experimenters can induce response conflict by presenting misleading stimuli and thus test conflict-induced adaptations of attentional weights. In task switching paradigms, participants must switch between multiple tasks or repeat them, allowing for a comparison of conditions in which control states that enable the execution of a specific task align with the current task demands or need to be adjusted. Finally, the existing body of theoretical work on cognitive control does not provide a conclusive prediction of the time course of control states. Empirical work on this topic has focused only on possibly nonabstract control, thus, rendering it unclear whether these findings can be generalized to abstract control.

1.2   Cognitive control and context

Numerous empirical and theoretical works have been concerned with context dependencies of cognitive control (e.g., Bugg, 2017; Crump et al., 2006; Dignath et al., 2019; Jiang et al., 2020). Although context is widely recognized to play a pivotal role in cognitive control (a frequently cited everyday example being that you would buzz the door in your own apartment's when the doorbells ring, whereas if the doorbells ring in someone else's apartment, you inhibit this action), there appears to be limited consensus regarding the precise definition of context. For example, Chiew and Braver (2017, p. 143) characterized context as "task-relevant information represented in such a form so as to bias selection of the appropriate task response". In contrast, Frings et al. (2023) defined context as a "task-irrelevant event that may not be constant throughout a stream of events and can alternate as the task-relevant features do". In this thesis, the focus will be on exploring the interplay between cognitive control and contexts uncorrelated with task demands (following the definition of Frings et al., 2023). However, to lay sufficient groundwork for the discussion, I will provide a brief overview of informative contexts and how they can instruct control demands. Importantly, unless explicitly stated otherwise, I will refer to context as task features that are task-irrelevant because they are uncorrelated with task demands, such as required responses, upcoming conflict, or required task rules.

1.2.1   Contexts that are informative about task demands

Studies investigating how cognitive control is affected by informative contexts typically add a context task feature to a paradigm used to measure control states. This context feature is manipulated independently of the original task features. For example, in a response interference task, the stimuli may be presented at the top or the bottom of the display. Here, the location serves as the manipulated context. Within each of these context levels, the probability of specific task demands can be manipulated. For instance, at the top location, mostly incongruent trials might be presented, whereas at the bottom location, mostly congruent trials would be presented. Studies have repeatedly demonstrated that participants show context-specific PCEs, that is, reduced CEs in trials with a mostly incongruent context compared to trials with a mostly congruent context (Corballis & Gratton, 2003; Crump, 2016; Crump et al., 2017; Crump et al., 2006; Vietze & Wendt, 2009; Weidler & Bugg, 2016; Wendt & Kiesel, 2011). Likewise, such effects of contextually cued cognitive control can

be found for cognitive flexibility in task switching, in which SCs are reduced if the context in which the task stimuli are presented is associated with an increased probability of task switches (Chiu & Egner, 2017; Crump & Logan, 2010; Leboe et al., 2008).

These effects have been interpreted as the results of learned associations between contexts and control states (Abrahamse et al., 2016). Encountering specific contexts triggers the reinstatement of the associated control state. This process has been successfully simulated by reinforcement learning models that change the locus of control from the task set units, as implemented by the original connectionist models of cognitive control (Cohen et al., 1990), to the specific connection weights between individual task features (Blais et al., 2007).

1.2.2   Contexts uncorrelated with task demands

Contexts that are not associated with specific task demands and consequently, that are not instructive for those also seem to play a significant role in the implementation of cognitive control. Dignath and Kiesel (2021; see also Yang et al., 2021) conducted an experiment using a paradigm similar to the one described in the previous section but eliminated the contingencies between the context and task demands. In other words, at both locations, the probability of encountering congruent or incongruent trials was 50%. Nevertheless, they observed the effects of conflict-induced control adaptations, in this case CSEs, were evident only when the stimuli in the control inducing previous trial and the control probing current trial occurred at the same location. Such a *context-specific CSE*[1] (C-CSE), defined as significantly smaller CSEs in context change sequences than in context repetition sequences, have been reported in multiple studies manipulating different types of context features which I will review in the following paragraph.

CSEs were found to be smaller when the format of the presented visual stimuli (e.g., digits or number words) changed across trials compared to when this context feature repeated (Dignath et al., 2019; Grant et al., 2021). The CSE decreased when visual features of the stimulus cueing the location of the target stimulus changed (Braem et al., 2014). Furthermore, CSEs were observed only if an auditory distractor word was spoken by the same voice in consecutive trials (Spapé & Hommel, 2008). Similarly, a CSE was reported only when the stimulus modality (auditory or visual) was repeated from the previous trial

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[1]The term *context-transition effect on the CSE* refers to the same effect and is equivalently used in some of the empirical studies that are displayed in Chapter III.

(Grant et al., 2020; Hazeltine et al., 2011). No CSE was observed when the flicker frequency of the stimuli changed across trials (Scherbaum et al., 2011). Lastly, changes in structural task features such as adding or removing a second task (Fischer et al., 2010), switching the temporal order of the distractor and the target stimulus (Dignath et al., 2021), or changing the task itself (Akçay & Hazeltine, 2011; Funes et al., 2010; Kiesel et al., 2006) could also elicit a C-CSE. It is worth noting that implementations of context-specific reinforcement learning (e.g., Blais et al., 2007) cannot account for the C-CSE, as there are no contingencies between context and task demands, that is, the probability of conflict trials.

Most of the research that has investigated the influences of contexts without contingencies to task demands on cognitive control has focused on response interference paradigms and the CSE as an index for control adaptations. In the related field of task switching, empirical tests examining whether abstract control states that support task performance show a similar context specificity as attentional weights are lacking. However, previous studies have begun to investigate the interplay between uninformative contexts and nonabstract task features, such as specific response codes. Several studies have demonstrated that task-specific benefits for response repetitions are diminished when uninformative context features changed across trials (Benini et al., 2022a, 2022b; Kandalowski et al., 2020; Koch, Frings, & Schuch, 2018; Schacherer & Hazeltine, 2022).

1.2.3 Interim summary

The preceding section has outlined how contexts with and without associations to specific task demands can impact cognitive control. It is crucial to differentiate between contexts with associations to specific task demands and those without such associations, as the mechanisms underlying their effects may differ. While the effects of informative contexts on control states can be modeled as a learning process, uninformative contexts do not permit the learning of context-specific control demands over multiple trials. In the following sections, I explore *episodic binding and retrieval* as potential mechanism underlying the C-CSE.

1.3 Episodic binding and retrieval

How do we know that the green leaves belong to the trunk of the tree and not to the sky in the background or to the grass that shares the same color? The feature integration theory (Treisman & Gelade, 1980) addresses the fundamental question of how we correctly extract objects with all their properties, such as shapes, colors, and motion, from our perceptual input—a challenge often referred to as the *binding problem* (Treisman, 1996). This theory proposes that attended visual features of objects, defined by their spatiotemporal contiguity, are bound into a transient, episodic representation known as an object file (Kahneman & Treisman, 1984; Kahneman et al., 1992). Object files seem to play a pivotal role in the guidance of behavior, as demonstrated experimentally by the facilitated identification of features that are part of an object file, especially when they occur at the same location (Kahneman et al., 1992).

The concept of object files originated from a background that strongly emphasizes the interplay of visual features and attention while historically neglecting its relations to action. This demarcation between visual attention and action has been prevalent in cognitive sciences (for discussions, see Humphreys et al., 2010; Neumann, 1990), although it was challenged already by William James, one of the pioneers in modern psychology. James noted instances where reactions seemed to follow almost reflex-like upon the perception of an object, while no conscious command to execute this action was expressed (James, 1890). Theories exploring this so-called *ideomotor principle* propose that the learning of contingencies between stimuli, actions and resulting action effects establish a bidirectional connection between the performed action and the perceived action effect. Under this principle, the boundaries between action and perception blur because actions are selected on the basis of their anticipated sensory feedback (Greenwald, 1970).

Empirical evidence supporting the ideomotor principle comes from experiments where the compatibility between an action and its expected action effect was manipulated. Participants showed better performance when the action was compatible with the expected action effect (e.g., a sound on the right side following a right-hand response), although the action preceded the action effect (e.g., Kunde, 2001). Furthermore, in two-stage experiments, participants first learned contingencies between actions and their effects. In the subsequent phase, the action effects were presented as target stimuli. Better performance was observed

when they required an action contingent to the associations learned in the first phase, supporting the ideomotor principle by indicating that the anticipated action effect can prime the associated action due to the bidirectional properties of these associations (e.g., Elsner & Hommel, 2001, 2004).

The ideomotor principle, which emphasizes the close connection between action and perception, has been incorporated into the *theory of event coding* (TEC; Hommel, 2019; Hommel et al., 2001) and integrated into a binding framework that goes beyond visual object files, as proposed by Kahneman et al. (1992). The core principle of TEC is the common coding principle for action and perception (Prinz, 1990). TEC posits that both share an identical representational medium (e.g., Hommel, 2009), extending the bidirectional link between action and perception proposed by ideomotor theories. The parsimony of this approach toward action control is well illustrated by actions such as head and eye movements that enable visual perception of the to-be-attended stimuli. For example, if one focuses on a moving object, the required motor pattern of the eyes is already defined by the visual input, that is, the movement of the attended object. According to TEC, these representations integrate perceived features, including feedback from actions on a common code. These shared representations of action and perception extend the binding problem (Treisman, 1996) since an object is described not only by its perceived features but also by the associated action-related features. To highlight that not only percepts such as objects can be represented but all types of events that can be described by their perceivable features, including actions, TEC proposes the term *event file* (Hommel, 1998).

TEC specifies additional assumptions. The theory assumes that the neural codes of event file features are distributed, meaning that there is no specific neural correlate for each event. Instead, each event is defined by a composition of specific neural codes that can be specific to features of the event. Further, event files are assumed to support action control by referencing to multimodal, distal features, that is, features of the external world, as opposed to proximal features, such as modal differences between features or the neural locations of features within the same representation. While TEC does not provide a clearly formalized, testable model (Hochberg, 2001; Oriet et al., 2001; Sanders, 2001), it makes a specific prediction of how event files influence behavior. According to the theory, if one or more bound features are reactivated, the retrieval of the event file should be triggered, meaning that the activation level of other bound features should increase. Consequently, if the features of the currently

experienced episode match the previously bound features, retrieval should improve performance. On the other hand, if the currently experienced features only partially match the bound features, the TEC predicts impaired performance, either due to activation of features unfit for the current episode or due to costs associated with weakening previous bindings (Mocke et al., 2023). This phenomenon is often referred to as *partial repetition costs* (Hommel, 1998).

1.3.1 Binding and retrieval in experimental research

The *binding and retrieval in action control* framework (BRAC; Beste et al., 2023; Frings et al., 2020) aims to apply the theoretical groundwork of the TEC to various paradigms commonly used in experimental psychology. BRAC emphasizes a theoretical approach to these paradigms that is structured along the two main processes proposed by the TEC: Binding and retrieval. To test the predictions derived from the TEC, it was suggested (Frings et al., 2020) to manipulate the conditions under which features might be bound to an event file (prime) and the conditions under which retrieval might occur (probe) independently. An example for this approach is the S1R1→S2R2 task (Hommel, 1998; see Fig. 3). Two responses were mapped to two stimuli. During the prime trial, participants are cued with one of the two possible responses, which they give when one of two stimuli is displayed. In the subsequent probe trial, either the same or a different stimulus from the prime is presented, and participants must respond according to the instructed mapping. This setup allows the manipulation of perceptual feature overlap (the stimulus identity) and action feature overlap (the given responses) independently. Consistent with the predictions of the TEC, these experiments typically reveal partial repetition costs, meaning that reactions are slowed down if the probe partially repeats the prime conjunction of stimuli and responses.

The basic logic of comparing task feature overlap between prime and probe can be applied to explain a range of behavioral effects in well-known action control paradigms. In the following, I will give some examples for such paradigms: In *negative priming*, performance is impaired if a stimulus ignored in the prime must be attended to in probe (Frings et al., 2015). In *repetition priming*, words can be better identified in the probe if features of the probe episode are repeated from the prime episode (Tenpenny, 1995). In *action planning*, in which an action is merely planned in the prime, responses are slowed down if the probe that comes between the prime and execution of the action planned in the prime partially repeats features of the prime (Kunde et al., 2002; Stoet & Hommel,
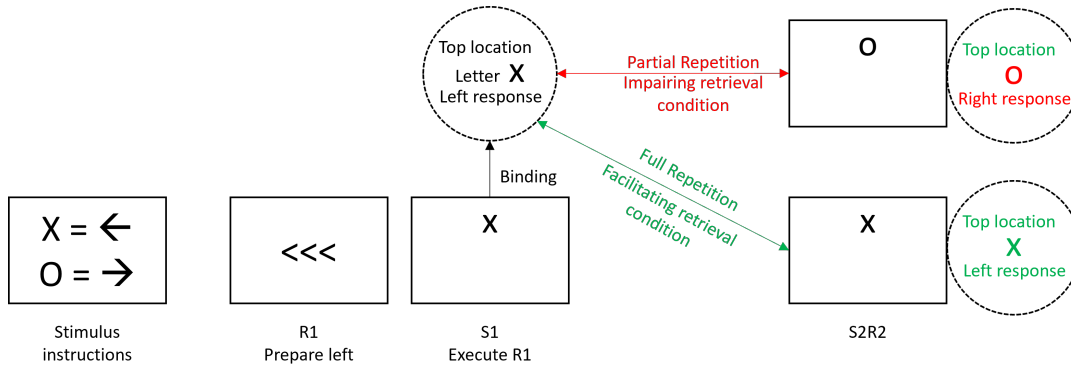
FIGURE 3: Feature binding and two exemplary retrieval conditions in the S1R1→S2R2 task (Hommel, 1998). Before the sequence, participants learn a stimulus-to-response mapping (in this case, x = left response; o = right response). During R1, participants are instructed to prepare a response. S1 acts as a go signal for the prepared response. All encountered trial features (stimulus location, stimulus, shape, response) are expected to be bound. In the subsequent S2R2 trial, the bound features can individually repeat or change. Facilitating retrieval conditions, reflected by good performance, are expected on trials in which all bound features are repeated. Imparing retrieval conditions, reflected by worse performance, are expected when the bound features are partially repeated.

1999). Furthermore, BRAC logic can explain effects in specific variations of *task switching* and *response interference* paradigms, as described in the following.

Schmidt and Liefooghe (2016; see also Schmidt et al., 2016) proposed that in task switching paradigms in which participants have to perform one of two discrimination tasks, SCs can largely be attributed to the effects of partial repetitions of the target, the response and/or the task cue. For instance, if the task cue and the response become bound in the prime trial, in task repetitions, the repeated task cue retrieves the associated response, thus resulting in faster responses.

Hommel et al. (2004) suggested that the CSE in the Simon task (Simon, 1990) might not reflect adaptations in attentional weights but rather be the result of partial repetition costs. In the Simon task, participants are instructed to respond with left or right button presses to a stimulus that is presented either on the left or right side of the screen. This results in congruent trials where the stimulus location indicates the same direction as the required response and incongruent in trials where the location and the required response mismatch. As described in an earlier section, congruency effects are influenced by congruency in the previous trial. Hommel et al. (2004) proposed that this effect might arise from differences in overlap conditions between stimulus and location features.

If all or no features of the prime are repeated in the probe, the performance is good. These conditions are necessarily congruent trials following a congruent trial or incongruent trials following an incongruent trial. If the probe partially repeats features of the prime performance is impaired. These conditions are necessarily congruent trials following an incongruent trial or incongruent trials following a congruent trial. This logic can account for the presence of CSEs in all response interference tasks with stimulus and response feature repetitions in succesive trials, especially designs using only two distractor and two target stimuli.

1.3.2   Binding context

Interestingly, such binding effects have been reported not only for task features that are directly relevant to successfully resolve the task but they are heavily influenced by context. From a TEC/BRAC perspective, two functions of context were discussed. First, variations in context could support the structure of event files by segmenting the stream of information processing (Qiu et al., 2023; see further Zacks et al., 2007). Second, context might act as a retrieval cue, indicating when it is appropriate to retrieve features that support action control. If previously bound context features are re-encountered, they retrieve the respective event file, improving performance if the retrieved features match current task demands but impairing performance if there is a mismatch. This is an often-observed effect for discrimination tasks with context features. For example, if participants are instructed to respond to the font color of a word but to ignore the meaning of the word, performance is impaired if, in the probe, the color of the required response is repeated but the word is different or if the word is repeated but not the response (e.g., Giesen & Rothermund, 2014; Rothermund et al., 2005; see also Frings et al., 2007; S. Mayr & Buchner, 2006). A similar effect was observed in task switching paradigms. Only if the probe repeats the prime task the typical response-repetition benefit is observed, that is, faster reactions in response repetitions than in response alternations (e.g. Kiesel et al., 2010). If context, such as varying stimulus modality (Benini et al., 2022b; Koch, Frings, & Schuch, 2018), visual features (Benini et al., 2022a), action effects (Schacherer & Hazeltine, 2022), or stimulus language (Benini et al., 2022b), is added to such a task, the task specific response repetition benefit is reduced if the context changes from prime to probe.

It should be noted that it is not trivial why contexts that are decorrelated

from any manipulated task demands are bound with other task-relevant features. Empirical studies suggest that contexts are only bound if they receive a sufficient amount of attention (Moeller & Frings, 2014). While contexts nominally provide no information for the selection of the correct response, participants may attribute some intrinsic relevance to them. Although not explicitly tested, potential reasons why participants attend to task-irrelevant feature dimensions of the current task could include that they must be processed along the target information (Dignath et al., 2019) or that they provide temporal information about target stimulus onset (Benini et al., 2022a; Benini et al., 2023).

1.3.3 Binding and retrieval of control states

TEC speculates that intentional control over actions comes from an additional mechanism referred to as intentional feature weighting (Hommel, 2019; Hommel et al., 2001; see also Memelink & Hommel, 2013). The basic idea is that intentions modulate the relative weights assigned to different features. For example, when deciding between two cups, features relevant to the decision, such as color, form, or size, receive increased weighting. Once a decision is made and the intention is to grab a specific cup, features relevant to this action, such as proprioceptive feedback or distance information, are weighted more strongly. Hommel et al. (2001) theorized that features with greater intentional weights have an increased base activation level. When stimulus processing activates these features, those with greater intentional weights are more likely to end up with a higher total activation than those with lower intentional weights. Since the activation of features after the processing of an event determines which features become bound, intentional weighting can exert action control a priori. It is worth noting that the concept of attentional weighting is very similar to more traditional concepts of cognitive control (for a comparison, see Cohen et al., 1990; and Memelink & Hommel, 2013).

Studies investigating bindings have focused primarily on observable features such as stimuli and actions (Frings et al., 2020). However, in a seminal study conducted by Spapé and Hommel (2008), it was observed that in an auditory Stroop task in which distractor words were presented auditorily, a CSE was present only if the voice that spoke the distractor word was repeated across trials. They reasoned that this C-CSE originated from implemented control states (Botvinick et al., 1999; Memelink & Hommel, 2013) that became bound to the context. Only under appropriate retrieval conditions, that is, repetitions

of the context that acts as a retrieval cue, these control states can be retrieved and influence the subsequent trial. Spapé and Hommel focused primarily on stimulus and response feature overlap between subsequent trials as the driving factor for the CSE. However, subsequent studies could replicate the C-CSE in confound-minimized response interference paradigms without stimulus or response repetitions across trials (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2020; see Fig. 4). This indicates that parameters controlling attentional weights independently from specific stimulus or response codes (i.e., abstract control states) can be bound to context features and can be subsequently retrieved upon reencounters of the respective context feature.

This novel approach that merges theoretical traditions from cognitive control research with episodic binding and retrieval accounts provides new perspectives on the long-standing discussion regarding the domain generality of cognitive control. Episodic binding and retrieval offers an explanatory framework for the question of why cognitive control typically cannot act across tasks with distinct demands; instead, the effects of control adaptations are usually observed to be task specific (see, e.g., Jiang & Egner, 2014; Kornblum, 1994; Notebaert & Verguts, 2008; Yang et al., 2021): Control states might become bound to their episodic context and could be retrieved only if this context is reencountered and can act as a retrieval cue. Consequently, the transfer of control states to a new context is prevented, thus reducing the danger of implementing control in an unsuitable environment.

1.3.4 Temporal continuity of bindings

Although binding theories do not formulate predictions about the temporal durability of bindings (Frings et al., 2020; Hommel, 2019, 2022; Hommel et al., 2001), this topic has been a significant focus of empirical work. Several studies implementing different types of task features as retrieval cues have manipulated the time interval between binding and retrieval trials. The majority of the studies report that binding effects decrease to zero within seconds, as reported for bindings between task-irrelevant stimuli and responses (Frings, 2011; Frings et al., 2022; Hommel & Frings, 2020; Moeller & Frings, 2017), task-relevant stimuli and responses (Frings et al., 2022; Hommel & Frings, 2020), and expected effects and responses (Moeller, Pfister, et al., 2016). This time course seemed to be invariant to possibly interfering events occurring within the intertrial interval, thus suggesting that temporal decay rather than interference is the mechanism
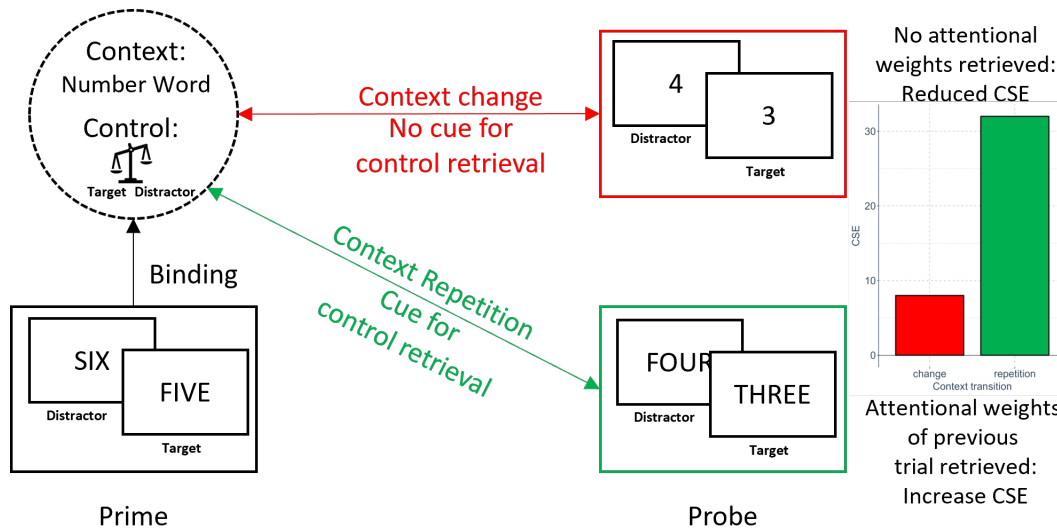
FIGURE 4: Control bindings as an explanation for the context-specific congruency sequence effect (C-CSE) in a contextualized prime-target task (results from Dignath et al., 2019). Participants are instructed to react to the second of two successive stimuli. Episodic binding and retrieval accounts suggest that in the prime, the adopted control state is bound to the context. In this example, a control state weighting the target over the distractor information was adopted due to the mismatching distractor information. This control state becomes bound to the format context "number word". Only if the context is repeated in the probe trial, the context can act as a cue to retrieve the bound control state. This is reflected by larger CSEs in context repetition than in context change sequences.

behind the short durability of binding effects (Hommel & Frings, 2020).

However, there is evidence suggesting that bindings can persist for longer periods in some cases. For example, bindings between two responses appear to be durable for at least six seconds (Moeller & Frings, 2021). Interestingly, studies have also reported that bindings between task-relevant stimuli and control states involving task sets and/or flexibility can be robustly observed after several trials and/or minutes (Moutsopoulou & Waszak, 2013; Pfeuffer et al., 2017; Whitehead et al., 2020, 2022). Consequently, the available literature suggests that the time course of bindings cannot be generalized across all types of bindings, that is, bindings including different types of features.

1.3.5 Interim summary

In the previous sections, I reviewed binding and retrieval accounts. These accounts posit that attended task features, that is, stimulus and response features as well as the control states that weight the different feature relevance for the present task, become bound together. Moreover, re-encountering any of the

bound features triggers a retrieval process that subsequently retrieves all other bound features. These accounts provide an explanation for why contexts that are not correlated to task demands can influence implemented control states. It is hypothesized that control states become linked to concurrently appearing contexts. Consequently, previously implemented control states can be retrieved only through appropriate retrieval cues—for instance, repetitions of the prior context.

Chapter 2
Objectives

In this thesis, I aim to explore three gaps in the body of literature focusing on the interplay between abstract control states and contextual variables.

First, theories on cognitive control lack a clear prediction of the expected time course of implemented control states (Botvinick et al., 2001; Gratton et al., 1992). Empirical work has suggested that adjustments in control states decay over time, but the authors of these studies focused on control adaptations that possibly acted on specific stimulus and response codes (Duthoo et al., 2014a; Egner et al., 2010). This finding aligns with observations from episodic binding and retrieval literature suggesting that stimulus-to-response bindings underly a decay function (Frings, 2011; Frings et al., 2022; Hommel & Frings, 2020; Moeller, Pfister, et al., 2016). However, it remains unclear which time course is to be expected for abstract control states acting independently from a specific stimulus and response code. Conversely, there is no literature on the expected time course of bindings between visual contexts and abstract control states. These questions are addressed in studies I and II.

Second, episodic binding and retrieval theories emphasize a specific retrieval process that is initiated upon reencountering a feature of a binding (Frings et al., 2020). However, studies supporting the notion of control binding and subsequent retrieval (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021; Spapé & Hommel, 2008) employ study designs in which control retrieval cannot be distinguished from disruption of the maintenance of adjusted control states due to context changes (Kreutzfeldt et al., 2016). In Study III, we employ a new task design to contrast these two accounts.

Third, effects of contextually bound abstract control states are only reported for response interference tasks (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021; Spapé & Hommel, 2008), but a generalization of such effects to tasks measuring control beyond attentional weighting is missing. For this reason, Study IV is concerned with contextually bound abstract task rules, thus broadening the body of binding effects observed in task switching (e.g., Benini et al., 2022a; Kandalowski et al., 2020).

2.1 Studies overview

Study I: Testing the time course of abstract and nonabstract control states.

Theories of cognitive control and control adaptations highlight that control states play a crucial role in translating goal representations into behavior (Botvinick et al., 2001; Cohen et al., 1992). A logical implication of these theories is that presently adopted control states must project into the future to serve a proactive function that surpasses mere reactive conflict resolution between goals and distracting information (Braver et al., 2009). Notably, prevalent theories of cognitive control do not provide specific predictions about the time course of control states (Botvinick et al., 2001; Gratton et al., 1992). Empirical research on the temporal dynamics of the CSE, functioning as a widely used measure of control adaptations, has shown that the CSE diminishes as the time intervals between control-inducing and control-probing trials increase, suggesting a rapid decay of control states (Duthoo et al., 2014a; Egner et al., 2010). However, these studies utilized a task design where stimuli and responses could repeat across trials, prompting the question of whether the control applied in these studies acted at the level of specific feature codes, such as at bindings between specific stimulus and response features (Hommel et al., 2004). Considering an episodic binding and retrieval perspective (Frings et al., 2020), one could draw parallels to studies investigating the time course of such stimulus-to-response bindings, which consistently indicate a decay function for such bindings (Frings, 2011; Frings et al., 2022; Hommel & Frings, 2020; Moeller & Frings, 2017). This raises the question of whether abstract control states that act independently from such specific stimulus and response codes underly the same decay function as nonabstract control states. To address this question, we employed a prime-probe task in a confound-minimized version, ensuring that CSEs in such a design must result from abstract control states, as there were no repetitions of stimulus or response features across trials. Additionally, we manipulated the time interval between the trials to test whether CSEs would decrease with increased delays, which would also indicate that abstract control states decay over time. In two further experiments, we contrasted such a task design with an identical design in which stimuli and responses could repeat across consecutive trials, providing a direct comparison of the time course of abstract and nonabstract control states. Finally, in a fourth experiment, we controlled for potential effects of task complexity, which could offer an alternative

explanation for the results observed in Experiments 1-3.

Study II: Testing the time course of abstract control states bound to visual contexts.

Study II focused on the question of the expected time course for bindings between abstract control states and visual contexts. Two opposing hypotheses were considered: Either bindings comprising visual context features would decay in a similar time period as bindings between stimuli and responses (Frings, 2011; Frings et al., 2022; Hommel & Frings, 2020; Moeller & Frings, 2017). Under this hypothesis, one would expect that the effects of uninformative contexts on implemented control states would diminish within a span of three to five seconds. Alternatively, one could speculate that bindings that comprise abstract control states might follow a different time course. This is because bound abstract features may need to persist for a longer duration for generalization to new environments (Badre et al., 2021). To contrast these accounts, we utilized a confound-minimized prime-target task, where the stimuli were displayed in one of two formats (either as number words or as digits). This nominally task-irrelevant context could repeat or change across trials. Such a design has been successfully used to induce context-to-control binding effects (Dignath et al., 2019). Additionally, we manipulated the time interval between the prime trials when control states became bound to the context and the probe trials where the effects of context-to-control bindings, operationalized as the C-CSE, were measured. We employed a Bayesian analytical approach to compare evidence supporting the temporal stability of context-to-control bindings against evidence suggesting temporal decay.

Study III: Testing the retrieval hypothesis in the context-specificity of the CSE.

Episodic binding and retrieval accounts propose specific mechanisms underlying the effects of context-to-control bindings, such as the C-CSE (Frings et al., 2020; Spapé & Hommel, 2008). According to these accounts, active control representations and active context representations become bound in the prime trial. In the subsequent probe trial, encountering the same context leads to retrieval of the control state bound to it. In probe trials, where the context does not repeat, the retrieval conditions are impaired, reducing the likelihood of successful control retrieval. However, prior empirical studies have focused on task designs in which the prime and probe were consecutive trials (N-1→N

designs; Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021; Spapé & Hommel, 2008). In such task designs, attentional reset offers an alternative explanation to the retrieval hypothesis (Kreutzfeldt et al., 2016). This account suggests that control states are actively maintained but that changes in context can disrupt this maintenance process. In Study III, we employed a task design specifically tailored to contrast these two accounts. We used a prime-target task and manipulated the display format of the stimuli (number words, digits, or dice symbols) acting as the context. We designed the experiments so that contexts never repeated across trials (N-1→N), but from N-2→N, the context could either repeat (ABA context sequences) or change (CBA context sequences). The retrieval account and the attentional reset account would make distinct predictions of N-2→N context transitions on measurements of control adaptations in N that were induced in N-2. The retrieval account predicts that in N-2 the adopted control state becomes bound to the context. Repeating this context in N should retrieve the bound control state, which would be reflected as larger N-2→N control adaptation effects on N-2→N context repetitions than on N-2→N context changes. In contrast, the attentional reset account solely makes predictions about sequential context changes on the maintenance of control states but does not predict an influence of the N-2→N context transition on N-2→N control adaptation effects. Using Bayesian statistics, we compared evidence for the retrieval hypothesis with evidence for the attentional reset hypothesis. In two further experiments, we controlled for other potential influences, such as response conflict in the intervening N-1 trial and response hand transitions across trials.

Study IV: Generalizing bindings of abstract control states to task control.

Previous studies that have explored bindings of uninformative contexts and abstract control states have typically operationalized control as attentional weighting through response interference tasks (e.g., Dignath et al., 2019; Dignath & Kiesel, 2021). Conversely, in the related research field of task switching, the influences of response bindings have been investigated (e.g., Benini et al., 2022a; Schuch & Keppler, 2022), but there has been a lack of empirical studies testing bindings that comprise abstract control states. In an approach to generalize effects of context-to-control bindings across different domains of cognitive control, we tested whether we could observe effects of bindings between abstract task rules (Kikumoto & Mayr, 2020; U. Mayr & Bryck, 2005) and visual

context features. To achieve this, we employed a task switching paradigm with three tasks that could not be distinguished based on their stimulus-to-response mappings. In such a design, any performance costs arising from task switches cannot be attributed to interference or lingering activation of specific stimulus and response features but instead originate from the implementation of new task rules. Additionally, a colorized pattern was displayed to serve as context during the performance of the tasks. According to the episodic binding and retrieval account, the active abstract task rules should become bound to the context. Repeating the context in the subsequent trial should facilitate the retrieval of these task rules, thereby enhancing performance when the same task is repeated but potentially hindering performance when a task switch is required.

Chapter 3

Studies

3.1 Study I: No Temporal Decay of Cognitive Control in the Congruency Sequence Effect

Moritz SCHILTENWOLF, Andrea KIESEL, and David DIGNATH

Cognitive control theories describe the active maintenance of goal representations over temporal delays as central for adaptive behavior. Dynamic adaptations of goal representations are often measured as the congruency sequence effect (CSE), which describes a reduced congruency effect in trials following incongruent trials compared to congruent trials. Previous studies questioned active maintenance of CSEs and instead found that CSEs decrease rapidly over time (Duthoo et al., 2014a; Egner et al., 2010). However, in these studies, CSEs can be attributed to both, control adaptations following conflict and binding effects due to repetition of stimulus (S) and response (R) features. In four experiments, we demonstrate that CSEs originating solely from control adaptions were not affected by temporal delays. Additional within-subject conditions partially replicated previous research showing a decrease in CSEs in task designs allowing for S-R binding effects and controlled for task complexity as a potential moderator. Together, results support theories which predict an active maintenance or retrieval of cognitive control.

Introduction

Cognitive control theories often emphasize the active maintenance of goal representations over temporal delays for goal-directed behavior (Badre & D'Esposito, 2007; Braver, 2012; Braver & Cohen, 2000; Koechlin & Summerfield, 2007). But how stable are adaptations in cognitive control that orchestrate attention and action in line with these goal representations? The present research investigates this question by using response-interference paradigms which measure the sequential modulations of the congruency effect (CSE) as a marker of dynamic control adaptations. The congruency effect is characterized by better performance in congruent trials (target and distractor match) compared to incongruent trials (target and distractor conflict). The CSE refers to reduced congruency effects in trials following incongruent compared to congruent trials (Gratton et al., 1992; for a review see Egner, 2007). Interestingly, the sequential task design used to assess CSEs introduces a temporal component, that is reflected in theoretical accounts which posit that control requirements from the current trial will affect behavior prospectively in the upcoming trial. However,

precise predictions about the actual time course of CSE are not always straight-forward, and empirical evidence how temporal delays between trials affect the CSE is limited.

Theoretical accounts of the CSE

An influential perspective that discusses the time-scale of control is the dual-mechanisms of control (Braver, 2012). Under this framework, a *proactive control* mode refers to the active and sustained maintenance of control. Regarding the CSE, proactive control describes the anticipation of upcoming conflict in the next trial and can be assumed to be active as long as conflict is expected. In addition, a *reactive control* mode refers to stimulus-driven, transient recruitment of control (Braver et al., 2009). Regarding the CSE, reactive control describes the control over conflict within the current trial which can persist until the next trial. In the following, we discuss four theoretical accounts and their respective predictions about the time-course of the CSE.

Congruency expectancy

Gratton et al. (1992) assumed that the CSE is caused by a biased, implicit expectancy about the congruency of the upcoming trial. According to this view, participants expect that the congruency of the current trial is repeated in the subsequent trial, which leads to a shift in processing weights between target and distractor information (Duthoo et al., 2013; Erb & Aschenbrenner, 2019; Jiménez & Méndez, 2013; but no control preparation elicited by explicit cues, see Jiménez et al., 2021). The expectancy view has been formally modelled within a Bayesian framework, hypothesizing that previous congruency level biases prior beliefs about upcoming congruency (Yu et al., 2009). Here, the prior expected probability of congruency frequencies is allowed to change across consecutive trials, causing faster and more correct responses for congruency level repetitions (e.g., incongruent incongruent) compared to changes (e.g., congruent incongruent). Although neither the original account nor the Bayesian model made specific predictions about the time-course across trials, subsequent research has interpreted the expectancy account in terms of a proactive control mode that prepares for upcoming stimulus configurations (van den Wildenberg et al., 2012; Duthoo et al., 2014a; Jiang et al., 2014). For instance, Egner and colleagues proposed that the expectancy account "views CSEs as resulting from an active, preparatory process, one would expect the effect to take some time to establish itself, and then to build up (or at least persist) over time in anticipation of the forthcoming stimulus" (Egner et al., 2010, p. 2).

Conflict monitoring

The Conflict Monitoring Theory explains the CSE as dynamic control adaptations regulated by a feedback loop which continuously monitors for conflict and intervenes accordingly (Botvinick et al., 2001). More specifically, if conflict is detected (e.g., during incongruent trials), control is recruited according to the strength of the conflict signal. As a consequence, after high conflict trials attention is biased towards target information, and away from distractor information. Correspondingly, after low conflict, congruent trials, relatively more weight is given to the distractor information, resulting in the CSE pattern. It is not straight forward to derive a definite prediction for the time course of the CSE from the Conflict Monitoring Theory (Egner et al., 2010) because the computational models discounts control not over time but for more distant trials (Botvinick et al., 2001). Subsequent research suggested both a proactive and a reactive interpretation for the CSE according to the model. Proponents of the proactive view emphasize that in the model, control adjustments prepare for upcoming stimuli, and therefore favored a proactive view (e.g., S. A. Wylie et al., 2010). Similar to the expectancy account, it seems reasonable to assume that a proactive reading of the conflict monitoring model predicts robust CSEs across longer delays. In contrast, proponents of the reactive view highlight the adjustment of cognitive control based on the current conflict, and describe the CSE as the result of the carry-over of residual reactive control emerging from conflict resolution in the previous trial (Scherbaum et al., 2012; Weichart et al., 2020). This reactive interpretation predicts that "conflict adaptation effects should become weaker when the inter trial interval is increased, since the carry-over of control adjustments decays" (Scherbaum et al., 2012, p. 127).

Stimulus and response binding

In the past, studies interested in CSEs often use task designs in which stimuli and responses (S-R) can repeat across consecutive trials. We will refer to these task designs as ´repetition´ designs. Critically, in ´repetition´ designs CSEs have been attributed to memory processes (Davelaar & Stevens, 2009; Hommel et al., 2004; U. Mayr et al., 2003). According to this perspective, co-occurring S-R features are bound together in episodic memory, and repetition of at least one of these S-R features in the next trial retrieves the previous episode (Frings et al., 2020; Hommel, 2004). For designs with small S-R sets, specific congruency transitions comprise of either the same/completely altered S-R combinations (e.g., congruent congruent) or partial repetitions/changes of

S-R feature (e.g., congruent incongruent) across trials. While the former case leads to more efficient performance, the latter case leads to retrieval of misleading S-R features, and thus impaired performance. As a consequence, it remains unclear to which extend CSEs in ´repetition´ designs reflect conflict adaption or S-R binding.

Which time course would be expected for a CSE purely driven by effects of S-R bindings? Several studies showed that effects of (partial) S-R repetitions decrease within a matter of seconds indicating that S-R bindings "can become functionally disintegrated after about five seconds" (Hommel & Frings, 2020, p. 755; Frings, 2011; Moeller, Pfister, et al., 2016). Accordingly, the S-R binding account would predict a fast temporal decrease of the CSE.

Control bindings

Recent literature has integrated reactive control accounts and S-R binding accounts to an overarching framework (Egner, 2014; Frings et al., 2020; Verguts & Notebaert, 2009). They assume that abstract, i.e. S-R independent, control parameters become bound to contextual cues such as location (Dignath & Kiesel, 2021), stimulus modality (Dignath et al., 2019; Grant et al., 2021) or task sets (Braem et al., 2014; Grant et al., 2020). Repetition of the context cues retrieves previously associated control parameter and gives rise to conflict adaptation effects. However, it remains unclear which time course for the CSE is to be expected from the control binding account. If bindings between context cues and abstract control parameters would decay as fast as bindings between concrete S-R features (see above), we would expect that the CSE decreases within a short time frame. In contrast, research on bindings between multiple actions found them to be temporally stable enabling the formation of more abstract action representations in which the linked features are more likely to be further apart in time (Moeller & Frings, 2021). Control theories have followed a similar line of reasoning, stating that "representations that are more abstract are relevant for longer" (Badre, 2008, pp. 198–199), and thus, one might speculate that bindings of abstract control parameters underly a more persistent time course than fast decaying S-R bindings (Hommel & Frings, 2020).

Previous research addressing the time-course of CSEs

In a seminal study, Egner and colleagues (2010; see also Duthoo et al., 2014a) found the CSE to decrease across time. In a Stroop task, Egner et al.

(2010) manipulated time intervals between trials, i.e. the inter-stimulus intervals (ISI) or the response-stimulus intervals (RSI) in steps between 500 to 5000 ms, whereby the ISI/RSI duration varied unpredictably for the participants. Results showed a rapid decrease in the CSE with increasing interval durations, until the complete absence of the CSE (but see Duthoo et al., 2014a Exp. 2, for temporally stable CSEs in an experiment with predictable RSI durations). The authors concluded that this CSE decrease was due to a rapid decay in reactive control adaptions. However, as explained above, the employed ´repetition´ design (repetitions of distractor and responses) makes it unclear whether temporal delays impaired the retrieval of S-R bindings or conflict adaptation. Indeed, a recent study found that effects of control adaptations induced by biased congruency proportions in the most recent trials did not change when comparing a 1000 ms and a 4000 ms delay (Colvett et al., 2020). Although this study did not investigate sequential control adaptations, i.e. CSEs, but list-wide control adaptations (se e.g. Bugg, 2012), it is relevant in the present discussion, because the trials inducing the control adaptation used a different S-R set than the trials probing the control adaptations. Such so-called ´confound-minimized´ task designs were also introduced for conflict tasks investigating sequential control adaptations and eliminate the effects of S-R binding on the CSE by avoiding S-R repetitions in sequential trials (Jiménez & Méndez, 2013; S. Kim & Cho, 2014; Schmidt & Weissman, 2014). As such, we suggest that it remains unclear whether previous studies (Duthoo et al., 2014a; Egner et al., 2010) observed a temporally decreasing CSEs due to decay of S-R bindings or due to temporal limitations of control adaptations.

Given this background, the present research aims to provide a further test of the stability of the CSE. The primary goal of our study was to examine the time-course of the CSE solely driven by control adaptations. In four experiments (all pre-registered), we used a ´confound-minimized´ prime-target task, i.e. free of S-R repetitions, and compared CSEs across different RSI levels (ranging from 1000ms to 9000 ms). RSI levels were manipulated pseudo-randomly from trial to trial making the duration of the RSI unpredictable for participants. We inquired whether the size of the CSE decreases for longer RSIs (as suggested by previous research using the ´repetition´ design, and predicted by reactive accounts of the CSE as well as accounts assuming fast decaying control bindings) or whether CSEs reflect more persistent control stable across longer temporal delays (as suggested by preliminary observations with the ´confound-minimized´ design and predicted by proactive accounts of the CSE as well as accounts assuming

temporally stable bindings of abstract action control). A secondary goal of our study was to replicate previous findings showing a decrease in CSEs in ´repetition´ designs that can be explained by both conflict adaptation and retrieval of bound S-R features, and compare it with the decrease in CSEs in ´confound-minimized´ designs. Experiment 2 and 3 therefore compared CSEs in both designs. Because ´repetition´ and ´confound-minimized´ designs differ in the complexity of S-R translations, Experiment 4 manipulated the number of S-R alternatives in the ´confound-minimized´ design and asked whether the decrease in CSEs differs as a function of S-R complexity.

Experiment 1

To probe CSEs that reflect control adaptations, we used a prime-target task with a "confound-minimized" design avoiding any S-R repetitions in sequential trials by using two S-R sets that alternated from trial to trial (e. g. in odd trials only S-R set A and in even trials only S-R set B). To test the temporal decrease of the CSE, the RSI was manipulated in steps of 1,000, 3,000, and 5,000 ms. The hypothesis, procedure, methods, and planned analysis were preregistered on the Open Science Framework (OSF, https://osf.io/zsp6w). All outlier criteria were also preregistered (identical for all experiments reported). Raw data, experiment scripts and analysis scripts are available on OSF.

Method

Participants

The smallest significant (one-sided) CSE difference between two RSI levels (as reported in Egner et al., 2010 and Duthoo et al., 2014) was dz = .4. In all three experiments, we tested more than 45 participants for a power of 90% (a = .05) to detect this effect. In Experiment 1, 50 participants (23 female, 27 male; age M = 30 years, range 18–70) recruited online via Prolific (Palan & Schitter, 2018) were tested. Participants were screened to have English as first language and to be right-handed. Participants with an overall error rate of 50% or an overall error rate of 3 SD higher than the sample mean were considered outlier. However, no participant fell under these criteria.

Task and stimuli

The participants used their private devices to run the experiment. The experiment was hosted on the department's webserver and was built with the

JavaScript library jsPsych (de Leeuw, 2015). A minimum browser window resolution of 1,280 3 700 px was required to participate in the experiment. Each trial presented a fixation cross, a prime, a blank, a target, and a blank response window (see Figure 5). The duration time for the fixation cross was dependent on the RSI condition (1,000 ms, 3,000 ms or 5,000 ms). The specific RSI levels were selected in accordance with a study by Hommel and Frings (2020) showing a linear temporal decay of S-R bindings. Target and prime stimuli were the numbers 3, 4, 5, and 6. In congruent trials, prime and target were identical, but in incongruent trials, the prime was different from the target. The prime stimulus was presented slightly larger than the target stimulus. The participants' task was to react to the displayed number by pressing the corresponding number key on their keyboard (3: index finger, 4: middle finger, 5: ring finger, 6: little finger; right hand). If participants did not respond during the target display or the response window, a 'too slow' feedback was displayed. On incorrect responses a "wrong" feedback was displayed. Feedback was displayed for 201 ms.

Procedure

After digitally providing informed consent, the participants received task instructions. The task was trained in a training block which also served as attention check. Participants who would fail to have an accuracy of at least 60% in the first 10 trials had to start again with reading the instructions. If they would fail the attention check again, the experiment would be aborted. After one training block the participants worked through ten blocks each containing every stimulus-condition combination once (48 trials). All participants were compensated with 3.75 £.

The "confound-minimized" design avoided any effects of S-R repetitions as well as negative priming effects by alternating between two different subsets of stimuli and responses across consecutive trials (e.g., numbers "3" and "4" were presented as prime and/or target in odd trials, numbers "5" and "6" were presented as prime and/or target in even trials, see e.g., Jiménez & Méndez, 2013; Schmidt & Weissman, 2014). Each participant was randomly assigned to one of three possible stimulus subset combinations. Each stimulus was shown equally often and paired an equal number of times with each congruency level, previous congruency level, and RSI duration. This procedure avoids contingency learning, that is, a learned association between certain stimuli and congruency levels (see Schmidt, 2013).
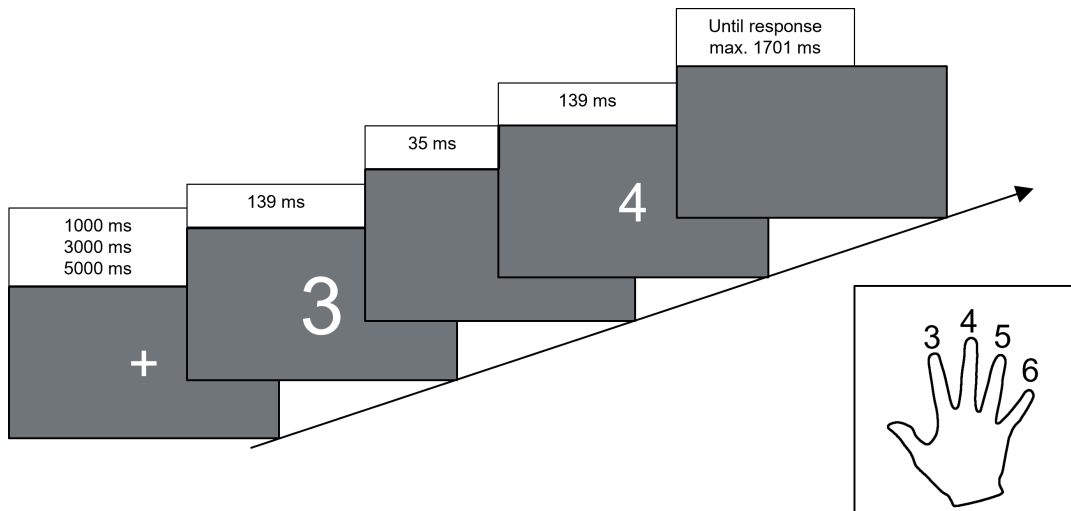
FIGURE 5: An example incongruent trial in the prime-target task deployed in Experiment 1. In each trial, a fixation cross was displayed for either 1000, 3000 or 5000 ms depending on the RSI condition. In the following, the prime and afterwards the target were displayed (intermitted by a short blank screen). The response window was terminated after a response was registered. The boxes above the trial components indicate the length of the respective component. The bottom right box indicates the stimuli-response mapping.

Analysis

First, to confirm CSEs in the short RSI condition, we analyzed mean RTs of the trials with 1000 ms RSIs using a repeated measures ANOVA with the factors current congruency [congruent vs incongruent] and previous congruency [congruent vs incongruent]. Second, to test whether CSEs are reduced with longer RSIs, we used a regression coefficient analysis (Pfister et al., 2013) to test if CSEs showed a negative slope over the three RSI conditions. For this reason, we conducted a linear regression for each participant with the RSI as predictor and CSE scores [$CSE = (meanRT_{con \to inc} - meanRT_{con \to con}) - (meanRT_{inc \to inc} - meanRT_{inc \to con})$] as criterion. A one-sided t-test against zero tested whether the regression coefficients were significantly less than zero. Standardized effect sizes (Cohen's d and η_p^2) are reported. To quantify evidence in favor for the null model, i. e. the test whether there was no temporal reduction of CSE, Bayes factors were computed using default prior width of .707. All analyzes were repeated with error rates.

In our preregistration protocol we planned to conduct an ANOVA with mean RTs as dependent variable and the factors current congruency, previous congruency and RSI. This analysis strategy would allow to test whether CSEs differ between *any* RSI condition. After data collection, we decided to deviate from the preregistration plan in favor of an analysis approach which better captures the hypothesis that we expect a linear decrease of the CSE over time. This regression coefficient analysis makes a clear prediction about the order of the conditional effects $[CSE(1000) > CSE(3000) > CSE(5000)]$ that is not captured by the ANOVA approach (in fact the ANOVA approach is most sensitive, if the effect between one factor level and every other factor levels is maximized; $F[mean_{conditionA} > (mean_{conditionB} = mean_{conditionC})] > F[mean_{conditionA} > mean_{conditionB} > mean_{conditionC}]$, if the difference between $mean_{conditionA}$ and $mean_{conditionC}$ is identical). The results of the originally preregistered ANOVA are reported in the Online Supplement. In order to quantify the alternative hypothesis that ´confound-minimized´ CSEs do not decrease over time, we added Bayes Factors to all critical analyses (CSE comparison between conditions and regression coefficient analyses). Please note that although not preregistered, these analyses are highly constrained by closely adhering to our initial hypothesis.

Results

According to the preregistration protocol, the first trial of each block (2.0 %) and trials following an incorrect trial (6.8 %) were discarded for all analyses. For RT analyses, also incorrect trials (6.7%) and trials with RTs exceeding 3 SD from the individual cell mean (1.1 %) were discarded. Mean RTs were calculated based on an average of 34 observations (SD = 3.6) per condition.

Reaction times

CSE analysis for the shortest RSI.   First, we asked whether the paradigm yields robust CSEs for the shortest RSI level. An ANOVA with the factors current congruency and previous congruency returned a main effect of current congruency, $F(1, 49) = 127.70, p < .001, \eta_p^2 = .723$, because RTs in incongruent trials were slower ($M = 818$ ms) than in congruent trials ($M = 705$ ms). Second, a main effect of previous congruency was observed, $F(1, 49) = 11.75, p = .001, \eta_p^2 = .193$, because RTs in trials following incongruent trials were slower ($M = 766$ ms) than in trials following congruent trials ($M = 757$ ms). Most importantly, a significant interaction between current congruency and previous congruency was observed, $F(1, 49) = 12.09, p = .001, \eta_p^2 = .198$,

TABLE 1: Mean RTs (in ms), Error Rates (in %), and CSEs for the Three RSI Conditions (Columns) in Experiment 1 and Mean Regression Slopes Predicting CSEs as a Function of the RSI Condition.

	RTs (ms)			Error rates (%)		
RSI duration:	**1000**	**3000**	**5000**	**1000**	**3000**	**5000**
Con. following con.	693	685	699	6.7	5.0	5.2
Inc. following con.	820	797	810	10.8	6.2	6.8
Con. following inc.	717	706	717	6.9	5.2	3.8
Inc. following inc.	815	792	801	9.6	5.9	5.0
CSE	29*	27*	28*	1.4	0.5	0.3
Mean regression equation for CSEs	$29.07 + (-0.47)xRSI$			$-1.82 + (0.04)xRSI$		

Note. Congruency sequence effects (CSEs) significantly different from zero (as indicated by t tests against zero) are denoted by *.

indicating that CEs were smaller in trials following incongruent trials ($\Delta = 98$ ms) compared to congruent trials ($\Delta = 127$ ms).

CSE analysis across RSI level.   Next, we asked whether the CSE changes with increasing RSIs. Figure 6 provides an overview of CSEs for each RSI level (RT, left side). A t-test showed that regression coefficients were not significantly less than zero, $t(49) = -.077, p = .470, d = -.01$. Bayes factors indicated moderate evidence for the null hypothesis, $BF_{01} = 6.494$, that CSE remain invariant across RSIs.

Error rates

CSE analysis for the shortest RSI.   The ANOVA for mean error rates returned a significant main effect for current congruency, $F(1, 49) = 21.01, p < .001, \eta_p^2 = .300$, because participant's error rates were higher in incongruent trials ($M = 10.2\%$) than in congruent trials ($M = 6.8\%$).

CSE analysis across RSI level.   A t-test against zero revealed no significant difference of the regression coefficients from the null model, $t(49) = -1.006, p = .160, d = -.142$. Bayes factors indicated moderate evidence for the null hypothesis, $BF_{01} = 4.032$.
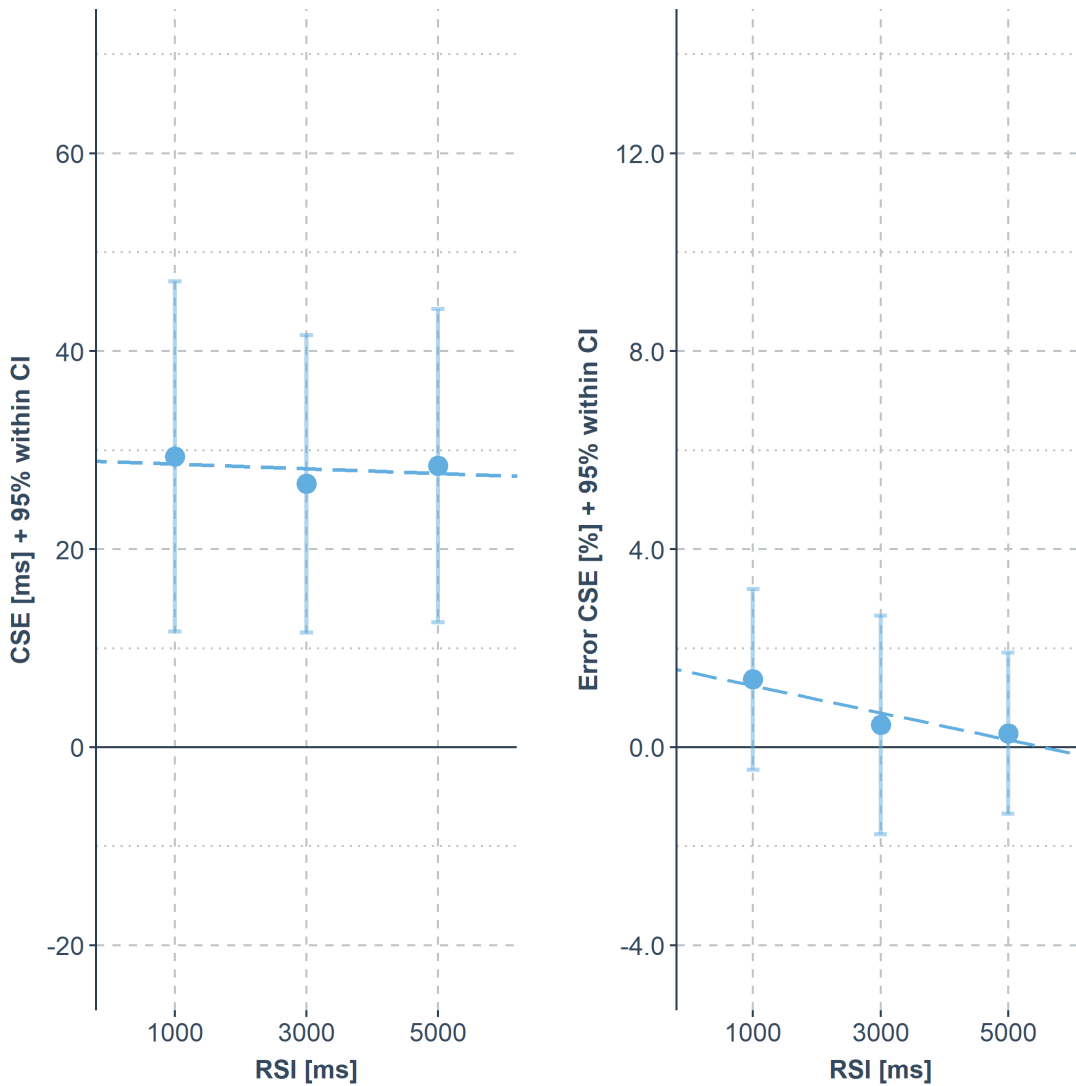
FIGURE 6: CSEs from Experiment 1 in RTs (left panel) and error rates (right panel) with the preceding RSIs on the x-axis. The dashed lines represent aggregated regression models with averaged intercepts and regression coefficients. Error bars indicate the 95% within confidence interval of the CSEs.

Discussion

Experiment 1 tested the time-course of the CSE in a ´confound-minimized´ prime-target task. Contrary to previous studies reporting transient CSEs, i.e. gradual decreasing CSE with increasing RSIs (Duthoo et al., 2014a; Egner et al., 2010), we did not observe any evidence for a decrease in CSE strength with increasing RSIs. Instead, Bayes Factors provide moderate evidence that CSEs remain stable across various time delays. This indicates that CSEs in the ´confound-minimized´ design reflect rather temporally persistent control parameter as suggested by proactive control accounts or binding accounts emphasizing that more abstract action control requires stability over longer time frames.

One way to account for these discrepant results might be due to the design employed, resulting in potential different contributions of control adaptations and S-R bindings to the CSE. Presumably, S-R bindings in the ´repetition´ design decay quickly, while control adaptations in the ´confound-minimized´ design extend across longer time delays. We test this speculation in Experiment 2.

Experiment 2

This experiment provides a replication of the ´confound-minimized´ design used in Experiment 1 and in addition, contrasts it with a ´repetition´ design. Participants always started with the ´repetition´ design and subsequently performed the ´confound-minimized´ design. Hypothesis, procedure, methods and planned analysis (but see deviation from the analysis plan explained above) were preregistered (https://osf.io/6x5zf). Raw data, experiment scripts and analysis scripts are available on OSF.

Method

Participants

We tested 45 participants (19 female, 26 male; age $M = 36$ years, range 19-65) via Prolific. The participation criteria were identical to those in Experiment 1, but participants of Experiment 1 and 3 were not able to take part. The data of three participants were excluded according to the preregistered outlier criteria. Two participants had an overall error rate of 50% or higher. From the remaining sample one participant had an overall error rate of 3 SD higher than the sample mean. All removed participants were replaced.

Task, stimuli and procedure

In the ´repetition ´ design participants responded to the numbers '8' and '9' using their index and middle finger. Please note that this condition allowed S-R bindings to influence the CSE (e.g., direct repetition of stimulus and response, partial repetitions, negative priming). Each prime-target combination was presented equally for each condition (e.g., congruency, previous congruency and RSI duration). The ´confound-minimized´ condition was as described in Experiment 1. After one training block, participants worked through seven blocks of the ´repetition´ design (24 trials per block) and another seven blocks

of the ´confound-minimized´ design (48 trials per block). Participants were compensated with 4.50 £.

Analysis

The same analysis strategy as in Experiment 1 was employed. In addition, we also included the within factor *design condition* [´repetition´ design vs ´confound-minimized´ design] to test whether CSEs in short RSI trials emerge in both conditions. To test whether CSEs change across RSI level, regression coefficients were calculated separately for the two design conditions using the same procedure as described in Experiment 1. To compare regression slopes between conditions, a one-sided paired-samples t-test was used.

Results

According to the preregistration protocol we excluded the first trial of each block (2.7 %), post-error trials (7.6 %) for all analyses and, in addition, error trials (7.5 %) and trial with response latencies exceeding 3 SD from the individual cell mean (0.7 %). Mean RTs were calculated based on an average of 13 observations ($SD = 1.9$) in the ´repetition´ design or on an average of 23 observations ($SD = 4.4$) in the ´confound-minimized´ design.

Reaction times

CSE analysis for the shortest RSI. The ANOVA resulted in the following effects. Two main effects were found. First, there was a main effect of current congruency, $F(1, 44) = 298.52, p < .001, \eta_p^2 = .872$, because RTs in incongruent trials were slower ($M = 718$ ms) than in congruent trials ($M = 601$ ms). Second, there was a main effect of design condition, $F(1, 44) = 146.28, p < .001, \eta_p^2 = .769$, because RTs in the ´confound-minimized´ design condition were generally slower ($M = 737$ ms) than in the ´repetition ´ design condition ($M = 583$ ms). Also, there were two significant two-way interactions. First, the interaction between current congruency and previous congruency, $F(1, 44) = 20.96, p < .001, \eta_p^2 = .323$, indicated a CSE with smaller CEs after incongruent ($\Delta = 99$ ms) compared to congruent trials ($\Delta = 135$ ms). Second, the interaction between current congruency and design condition, $F(1, 44) = 10.85, p = .002, \eta_p^2 = .198$, indicated that CEs were smaller in the ´repetition´ design condition ($\Delta = 93$ ms) than in the ´confound-minimized´ design condition ($\Delta = 141$ ms). The three-way interaction was not significant, $F(1, 44) = .28, p = .602, BF_{01} = 5.434$.

CSE analysis across RSI level. Regression coefficients in the ´repetition´ design did not differ significantly from zero, $t(44) = 1.070, p = .855, d = .159$, with moderate evidence for the null model, $BF_{01} = 3.623$. For the ´confound-minimized´ design regression coefficients also did not differ significantly from zero, $t(44) = 0.825, p = .793, d = .123$, with moderate evidence for the null model, $BF_{01} = 4.505$. Regression slopes did not differ significantly between conditions, $t(44) = 0.053, p = .521, d = -.008$, with moderate evidence for the null model, $BF_{01} = 6.182$.

Error rates

CSE analysis for the shortest RSI. For mean error rates the ANOVA revealed three main effects. First, there was a main effect of current congruency, $F(1, 44) = 19.35, p < .001, \eta_p^2 = .305$, because participants committed more errors in incongruent ($M = 10.7\%$) than congruent trials ($M = 5.7\%$). Second, the main effect of previous congruency, $F(1, 44) = 5.33, p = .026, \eta_p^2 = .108$, indicated that error rates were higher after congruent ($M = 9.0\%$). than after incongruent trials ($M = 7.4\%$). Third, there was a main effect of design condition, $F(1, 44) = 5.05, p = .030, \eta_p^2 = .103$, because in the ´confound-minimized´ design participants had overall higher error rates ($M = 9.8\%$) than in the ´repetition´ design ($M = 6.6\%$). The three-way interaction between current congruency, previous congruency and design condition was significant, $F(1, 44) = 5.78, p = .021, \eta_p^2 = .116, BF_{01} = 0.471$. Follow-up ANOVAs (with the factors current congruency and previous congruency) conducted separately for the ´repetition´ design and the ´confound-minimized´ design indicated that only in the ´repetition´ design error CSEs emerged, $F(1, 44) = 5.62, p = .022, \eta_p^2 = .113$, but not in the ´confound-minimized´ design condition, $F(1, 44) = 1.46, p = .233, \eta_p^2 = .032$.

CSE analysis across RSI level. Regression coefficients for the ´repetition´ design differed significantly from zero, $t(44) = -1.973, p = .027, d = -.294$, with an undecisive Bayes Factor indicating anecdotal evidence for the null model, $BF_{01} = 1.055$. For the ´confound-minimized´ design regression coefficients did not differ significantly from zero, $t(44) = 0.333, p = .630, d = .050$, with moderate evidence for the null model, $BF_{01} = 5.882$. Regression slopes differed marginally between conditions, $t(44) = -1.434, p = .079, d = -.214$, with anecdotal evidence for the null model, $BF_{01} = 2.389$.

TABLE 2: Mean RTs (in ms), Error Rates (in %), and CSEs for the Three RSI Conditions (Columns) in Experiment 2 and Mean Regression Slopes Predicting CSEs as a Function of the RSI Condition.

	RTs (ms)			**Error rates (%)**		
RSI duration:	**1000**	**3000**	**5000**	**1000**	**3000**	**5000**
Confound-minimized						
Con. following con.	653	664	676	8.4	5.3	5.9
Inc. following con.	809	805	810	11.7	9.4	8.3
Con. following inc.	680	687	712	6.9	4.2	5.2
Inc. following inc.	805	799	802	12.1	8.3	8.9
CSE	32*	28*	43*	-2.0	0.0	-1.2
Mean regression equation for CSEs	$22.63 + (5.90)xRSI$			$-1.82 + (0.04)xRSI$		
Repetition						
Con. following con.	526	498	521	4.0	1.4	1.9
Inc. following con.	639	621	621	11.8	7.1	5.0
Con. following inc.	545	534	552	3.4	1.7	2.4
Inc. following inc.	620	617	601	7.1	5.3	5.4
CSE	39*	40*	51*	4.1*	2.2*	0.0
Mean regression equation for CSEs	$30.63 + (5.90)xRSI$			$6.17 + (-2.03)xRSI$		

Note. Congruency sequence effects (CSEs) significantly different from zero (as indicated by t tests against zero) are denoted by *.
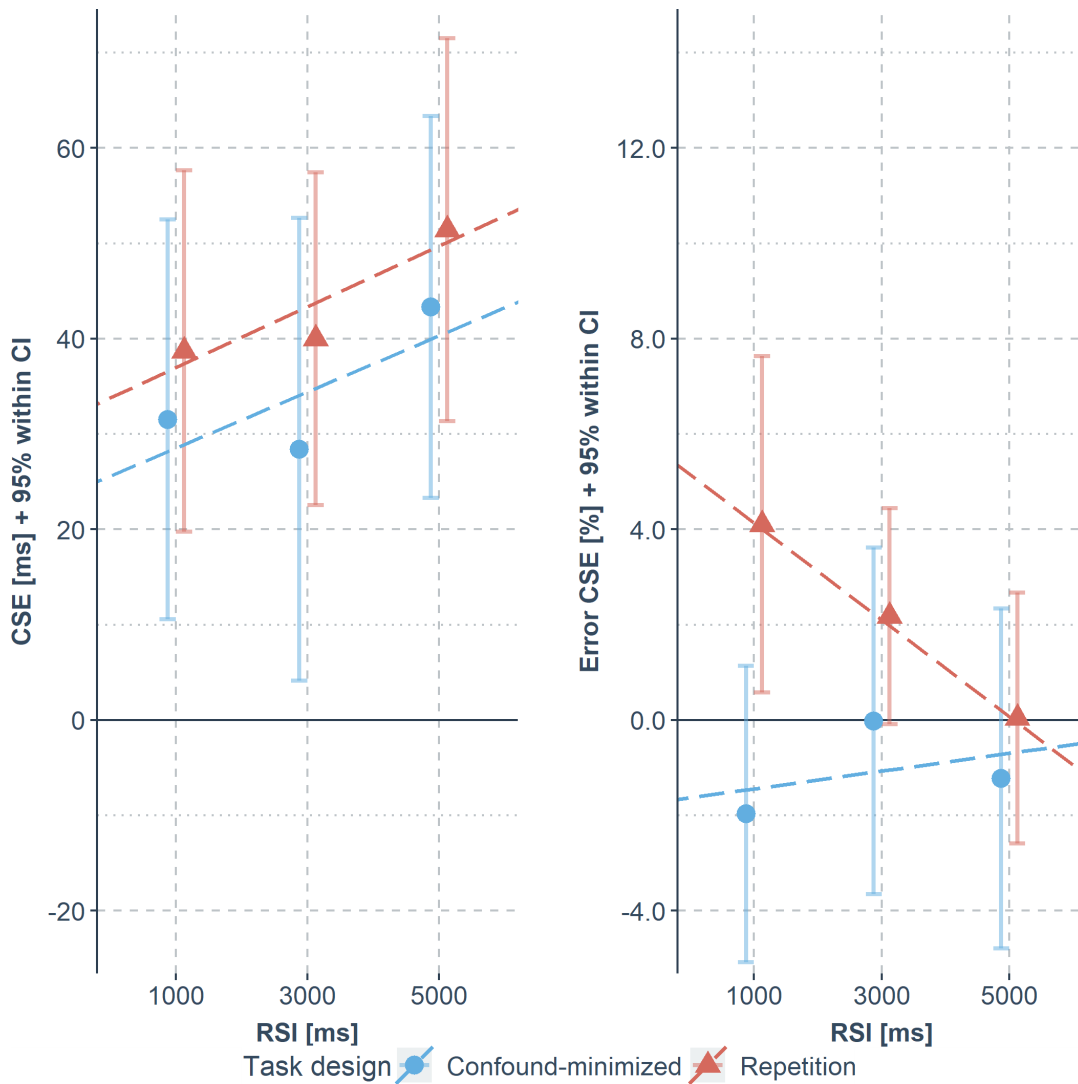
FIGURE 7: CSEs from Experiment 2 in RTs (left panel) and error rates (right panel) with the preceding RSIs on the x-axis. The dashed lines represent aggregated regression models with averaged intercepts and regression coefficients. Error bars indicate the 95% within confidence interval of the CSEs.

Discussion

Experiment 2 had two goals. First, we wanted to provide a further test of the temporal stability of the CSE in the ´confound-minimized´ design. Results replicated CSEs which did not decrease across different RSIs, providing further evidence for persistent control adaptations. Second, we wanted to compare the temporal stability of CSEs between the ´confound-minimized´ design and the ´repetition´ design. While the former taps more into sustained control adaptations or retrieval of abstract control parameters, the latter confounds their contributions to the CSE with influences of concrete S-R bindings. Here, results were mixed. In RTs, CSE remained invariant across RSIs for both designs. This finding is surprising given previous research that found a gradual

decrease of the size of the CSE for longer RSIs (Duthoo et al., 2014a; Egner et al., 2010). Although we replicated this decrease in the error data for the ´repetition´ design, we think this provides only limited support for the hypothesized decay of S-R bindings, since Bayes Factors remained undecisive and we had no a priori expectations to find this effect in error rates only. Further, in error rates, the comparison between the conditions is difficult, because in the ´confound-minimized´ design CSEs emerged only in RTs and not error rates.

Experiment 3

Given the unexpected outcome of Experiment 2, a replication is required. Furthermore, a potential limitation of Experiment 2 is due to possible order effects, since the ´repetition´ design always preceded the ´confound-minimized´ design. Therefore, Experiment 3 randomized the order of condition between participants to control for potential confounds. Hypothesis, procedure, methods and planned analysis were preregistered (https://osf.io/hdtxm). Raw data, experiment scripts and analysis scripts are available on OSF.

Method

Participants

We tested 49 participants (1 diverse, 29 female, 19 male; age $M = 29$ years, range 18-47) via Prolific. The participation criteria were identical to those in the previous experiments, but participants of Experiment 1 and 2 were not allowed to take part. Data of four participants were excluded according to the preregistered outlier criteria. Three participants had an overall error rate of 50% or higher. From the remaining sample one participant had an overall error rate of 3 SD higher than the sample mean. All removed participants were replaced.

Task, stimuli and procedure

Experiment 3 was identical to Experiment 2, with the only exception that we randomized the order in which participants worked on the ´repetition´ design and the ´confound-minimized´ design. Participants were compensated with 5.50 £.

Analysis

The same analysis approach as in Experiment 2 was used, with the following exception. To statistically control for potential order effects, we added the between factor design condition order [repetition→confound-minimized vs confound-minimized→repetition] to the ANOVA model, to test whether CSEs in short RSI trials emerge in all conditions. For the regression coefficient analysis, we used an ANOVA with the within factor design condition and the between factor design condition order to compare slopes between conditions.

Results

According to the preregistration protocol we excluded the first trial of each block (2.7 %), post-error trials (5.8 %) for all analysis and, in addition, error trials (5.7 %) and trials with response latencies exceeding 3 SD from the individual cell mean (0.8 %). Mean RTs were calculated based on an average of 13 observations ($SD = 1.6$) in the ´repetition´ design or on an average of 24 observations ($SD = 3.0$) in the ´confound-minimized´ design.

Reaction times

CSE analysis for the shortest RSI. The ANOVA yielded three main effects. First, there was a main effect of current congruency, $F(1,47) = 533.23$, $p < .001, \eta_p^2 = .919$, because participants responded slower in incongruent trials ($M = 684$ ms) than congruent trials ($M = 565$ ms). Second, the main effect of previous congruency was significant, $F(1,47) = 5.52, p = .023, \eta_p^2 = .007$, because RTs in trials following incongruent trials were slower ($M = 662$ ms) compared to those following congruent trials ($M = 654$ ms). Third, there was a main effect of design condition, $F(1,47) = 159.25, p < .001, \eta_p^2 = .772$, because in the ´confound-minimized´ design RTs were slower ($M = 702$ ms) than in the ´repetition´ design ($M = 547$ ms). Further, two significant two-way interactions were observed. First, there was an interaction of current congruency and previous congruency, $F(1,47) = 77.14, p < .001, \eta_p^2 = .621$, indicating a significant CSE with smaller CEs after incongruent ($\Delta = 94$ ms) than after congruent trials ($\Delta = 143$ ms). Second, there was an interaction of current congruency and design order, $F(1,47) = 11.83, p = .001, \eta_p^2 = .201$, indicating that participants who worked through the experiment in the order repetition→confound-minimized showed smaller CEs ($\Delta = 101$ ms) than those who worked through the experiment in the order confound-minimized→repetition ($\Delta = 136$ ms). The three-way interaction between of current congruency,

previous congruency and design condition which would indicate CSE differences between the ´confound-minimized´ and the ´repetition´ design was not significant, $F(1, 47) = 3.62, p = .063, BF_{01} = 1.217$.

CSE analysis across RSI level.   Individual regression slopes were submitted to the ANOVA model which revealed only a main effect of design condition, $F(1, 47) = 6.59, p = .014, \eta_p^2 = .123$, because regression slopes were more negative in the ´repetition´ design ($M = -17.29$) then in the ´confound-minimized´ design ($M = 2.48$) with Bayes Factors suggesting anecdotal evidence for the H1 for this comparison, $BF_{01} = 0.354$. As in previous analysis, we also tested whether regression slopes in each design showed a linear decrease of the CSE, i.e., differed negatively from zero. In the ´repetition´ design CSE regression slopes were significantly less than zero, $t(48) = -3.094, p = .002, d = -.442$, Bayes Factors indicated strong evidence in favor of the alternative hypothesis, $BF_{01} = 0.100$. In the ´confound-minimized´ design CSE regression slopes were not significantly different from zero, $t(48) = .488, p = .686, d = .070$, and Bayes Factors suggest moderate evidence in favor of the null model that the size of CSEs does not change across RSI level, $BF_{01} = 5.747$.

Error rates

CSE analysis for the shortest RSI.   The ANOVA for mean error rates revealed two main effects. First, there was a main effect of current congruency, $F(1, 47) = 39.38, p < .001, \eta_p^2 = .456$, because participants committed more errors on incongruent ($M = 9.5\%$) than on congruent trials ($M = 4.2\%$). Second, there was a main effect of task condition, $F(1, 47) = 4.96, p = .031, \eta_p^2 = .095$, because participant's error rates were higher in the ´confound-minimized´ ($M = 7.8\%$) compared to the ´repetition´ design condition ($M = 6.1\%$). Also, four two-way interactions were observed. First, we observed an interaction for the factors current congruency and previous congruency, $F(1, 47) = 14.84, p < .001, \eta_p^2 = .240$, indicating an error CSE with smaller CEs after incongruent ($\Delta = 3.4\%$) than after congruent trials ($\Delta = 7.3\%$). Second, there was an interaction between congruency and task condition, $F(1, 47) = 15.80, p < .001, \eta_p^2 = .252$, indicating that error CEs were smaller in the ´confound-minimized´ condition ($\Delta = 2.6\%$) than in the ´repetition´ condition ($\Delta = 8.0\%$). Third, there was an interaction between current congruency and task order, $F(1, 47) = 4.80, p = .034, \eta_p^2 = .093$, indicating that error CEs were smaller in participants in the confound-minimized→repetition condition ($\Delta = 7.2\%$) than in the repetition→confound-minimized ($\Delta = 3.5\%$). Fourth, there was an interaction

between task condition and task order, $F(1, 47) = 8.84, p = .054, \eta_p^2 = .158$, because for participants in the repetition→confound-minimized condition trials in the ´confound-minimized´ condition were more error prone than trials in the ´repetition´ condition ($\Delta = 3.55\%$) while this relation switched for participants in confound-minimized→repetition condition ($\Delta = -0.51\%$). Further, there was also a three-way interaction between the factors current congruency, previous congruency and task condition, $F(1, 47) = 10.09, p = .003, \eta_p^2 = .177, BF_{01} = .103$. Two follow-up ANOVAS with the factors current congruency and previous congruency revealed only a significant error CSE for the ´repetition´ design ($F(1, 48) = 18.95, p < .001, \eta_p^2 = .283$) and none for the ´confound-minimized´ condition ($F(1, 48) = 0.30, p = .589, \eta_p^2 = .006$).

CSE analysis across RSI level. The ANOVA produced only the two-way interaction for design condition and design condition order, $F(1, 47) = 4.31, p = .043, \eta_p^2 = .084$, indicating that the difference in the size of the regression coefficients between the ´confound-minimized´ and the ´repetition´ condition was less for subjects in the order condition repetition→confound-minimized ($\Delta = -0.01$) than for subjects in the confound-minimized→repetition condition ($\Delta = 0.05$). Further, we tested whether regression slopes in each design differed from zero. In the ´repetition´ design CSE regression slopes significantly differed from zero, $t(48) = -2.124, p = .019, d = -.303$. Bayes Factors indicated moderate evidence in favor of the alternative hypothesis, $BF_{01} = 0.124$. In the ´confound-minimized´ design no significant difference from zero was observed, $t(48) = .202, p = .580, d = .029$. Bayes Factors suggested anecdotal evidence in favor of the null model that CSE remain invariant across RSIs, $BF_{01} = 2.398$.

Discussion

Experiment 3 again showed that CSEs are temporally stable across RSIs in the ´confound-minimized´ design. Furthermore, results from the ´repetition´ design replicated findings from other research groups (Duthoo et al., 2014a; Egner et al., 2010) showing that CSEs decreased with increased RSIs. The error data is largely consistent, but additionally suggests that the order of the task design could moderate the decrease of the CSE. Although this order effect might be potentially interesting for future research, we are hesitant to draw stronger conclusions from this interaction because we had no hypothesis for an interaction with the factor order and the present sample size is probably too small for a reasonable between-groups comparison.

T̴ABLE 3: Mean RTs (in ms), Error Rates (in %), and CSEs for the Three RSI Conditions (Columns) in Experiment 3 and Mean Regression Slopes Predicting CSEs as a Function of the RSI Condition.

	RTs (ms)			Error rates (%)		
RSI duration:	**1000**	**3000**	**5000**	**1000**	**3000**	**5000**
Repetition→Confound-minimized						
Confound-minimized						
Con. following con.	637	612	619	7.0	6.2	4.3
Inc. following con.	751	723	736	7.0	6.7	7.7
Con. following inc.	656	647	653	7.7	7.2	3.7
Inc. following inc.	742	737	730	8.0	6.6	6.3
CSE	28*	22	40*	2.2	1.4	-0.3
Mean regression equation for CSEs	$17.87 + (6.03)xRSI$			$3.50 + (-1.20)xRSI$		
Repetition						
Con. following con.	482	483	498	1.5	1.2	0.9
Inc. following con.	618	610	597	9.6	8.7	6.6
Con. following inc.	524	500	511	1.9	1.2	2.1
Inc. following inc.	593	590	598	4.8	5.9	3.3
CSE	67*	38*	12	5.2*	2.8	4.5*
Mean regression equation for CSEs	$93.51 + (-27.36)xRSI$			$4.80 + (-0.31)xRSI$		
Confound-minimized→Repetition						
Confound-minimized						
Con. following con.	620	624	635	3.1	5.4	4.9
Inc. following con.	788	770	771	8.8	6.0	7.8
Con. following inc.	652	664	673	5.0	5.4	3.9
Inc. following inc.	773	749	759	9.4	4.8	6.0
CSE	47*	60*	46*	-0.9	1.7	2.5
Mean regression equation for CSEs	$52.81 + (-0.93)xRSI$			$-2.26 + (1.70)xRSI$		
Repetition						
Con. following con.						
Inc. following con.	609	604	612	16.0	4.8	5.7
Con. following inc.	494	500	498	3.9	1.4	1.3
Inc. following inc.	597	590	599	9.8	5.9	3.5
CSE	51*	47*	37*	9.2*	-1.0	1.7
Mean regression equation for CSEs	$58.90 + (-7.09)xRSI$			$10.81 + (-3.76)xRSI$		
Independent from condition-order						
Confound-minimized						
Con. following con.	629	618	627	6.2	5.5	3.7
Inc. following con.	769	746	753	9.1	6.5	7.2
Con. following inc.	654	655	660	6.4	6.3	3.8
Inc. following inc.	757	743	744	8.7	5.7	6.2
CSE	37*	41*	43*	0.7	1.6	1.1
Mean regression equation for CSEs	$34.99 + 2.62xRSI$			$0.7 + 0.2 * RSI$		

Note. Congruency sequence effects (CSEs) significantly different from zero (as indicated by t tests against zero) are denoted by *.
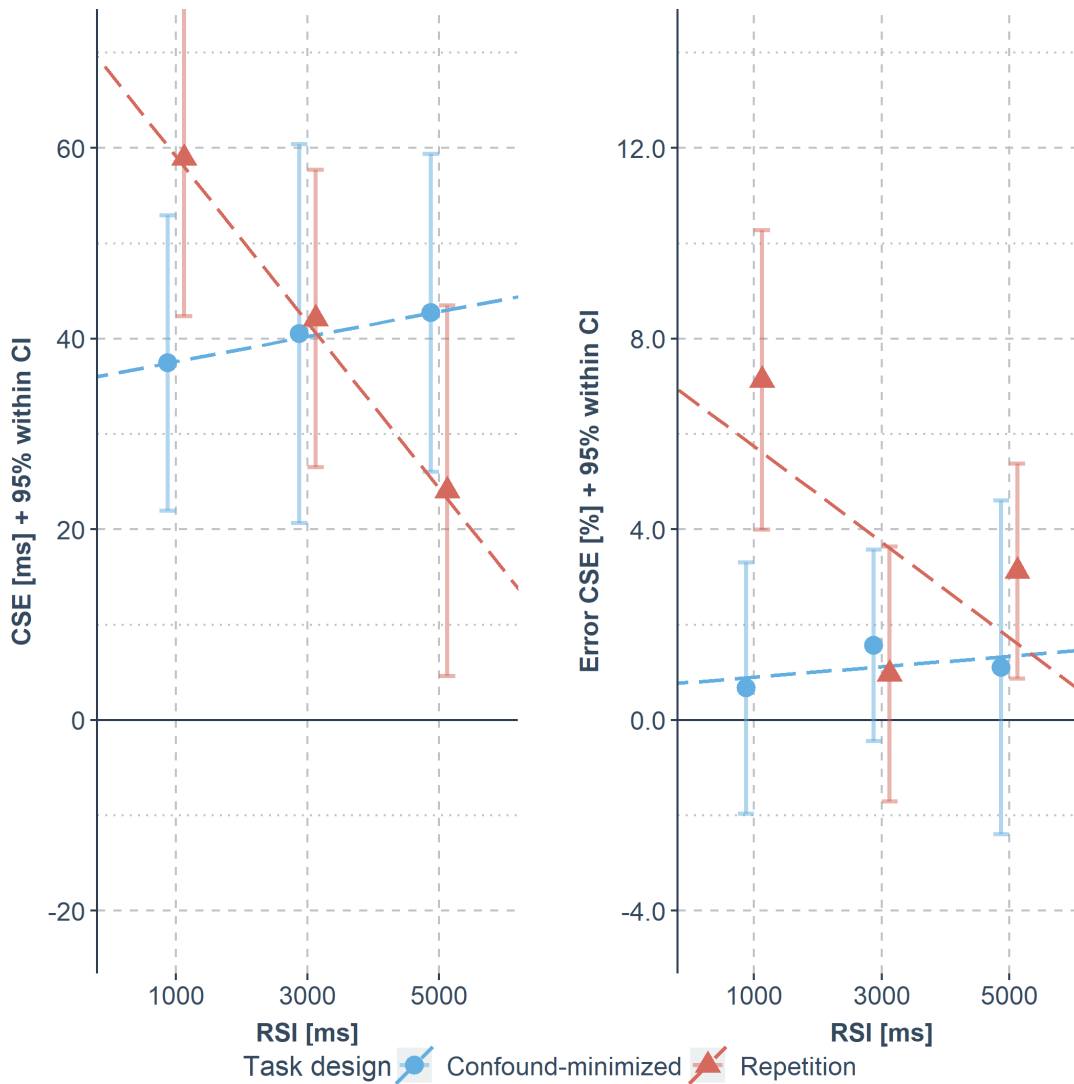
FIGURE 8: CSEs from Experiment 3 in RTs (left panel) and error rates (right panel) with the preceding RSIs on the x-axis. The dashed lines represent aggregated regression models with averaged intercepts and regression coefficients. Error bars indicate the 95% within confidence interval of the CSEs.

Experiment 4

The ´repetition´ and the ´confound-minimized´ designs as employed in Experiment 2 and 3 differ not only in term of S-R repetitions but also in the number of S-R sets. This poses a possible confound due to differences in task-complexity, because in the ´confound-minimized´ design participants had to keep an additional S-R set active, which in turn might modulate the temporal stability of control adaptations. To test whether task-complexity affects the time course of control adaptations, Experiment 4 compares two ´confound-minimized´ tasks, one with a complexity of four stimuli and responses ('4 S-R') and one with six stimuli and responses ('6 S-R'). Furthermore, since Experiments 1-3 showed

no CSE decrease in ´confound-minimized´ task designs with RSIs up to 5000 ms, RSI intervals were increased up to 9000 ms to provide a stronger test of the temporal stability of ´confound-minimized´ CSEs. Hypothesis, procedure, methods and planned analysis were preregistered (https://osf.io/struj). Raw data, experiment scripts and analysis scripts are available on OSF.

Method

Participants

67 participants (30 female, 36 male, 1 did not provide gender information; age $M = 38$ years, range 18-70) were tested via Prolific. Sample size rational was based on a power analysis to find an effect of dz = .364 (as indicated by the difference in the CSE regression coefficients between the ´confound-minimized´ and ´repetition´ design for RTs in Experiment 3) with a test power of 90% (α = .05). The participation criteria were identical to those in Experiment 1, but participants of Experiment 1, 2 and 3 were not able to take part. The data of one participant was excluded according to the preregistered outlier criteria, due to an overall error rate of 3 SD higher than the sample mean. The removed participant was replaced.

Task, stimuli and procedure

The '4 S-R' task was identical to the ´confound-minimized´ design in Experiments 1-3 with the difference that two additional RSI conditions were added (RSI levels: 1000 ms, 3000 ms, 5000 ms, 7000 ms, 9000 ms) and the target stimuli were '5', '6', '7' and '8'. In the '6 S-R' task the stimuli '3' and '4' were added. Participants responded with their left middle and index finger to the additional stimuli. In both task conditions, sequential trials came from different sub-sets of stimuli. The sequence of subsets was fixed (in the '4 S-R' task ['5', '6'] and ['7', '8'] alternated; in the '6 S-R' task the subset sequence was always ['3', '4'] → ['5', '6'] → ['7', '8'] → ['3', '4'] etc.). Participants worked through four blocks of the ´4 S-R´ design (80 trials per block) and three blocks of the ´6 S-R´ design (120 trials per block). Participants were compensated with 7.16 £.

Analysis

The same analysis strategy as in Experiment 2 was employed. Instead of the within factor *design condition* the within factor *complexity condition* ['4 S-R' vs '6 S-R'] was included.

Results

According to the preregistration protocol, we excluded the first trial of each block (1.0 %), post-error trials (7.9 %) for all analyses and, in addition for the RT analyses, error trials (7.8 %) and trials with response latencies exceeding 3 SD from the individual cell mean (0.2 %). Mean RTs were calculated based on an average of 14 observations ($SD = 2.1$) in the ´4 S-R´ design or on an average of 15 observations ($SD = 2.8$) in the ´6 S-R´ design. Please note that due to time constraints that came with the longer RSI conditions the number of observations per RSI level were reduced in comparison to Experiment 2 and 3 while the overall number of observations was increased.

Reaction times

CSE analysis for the shortest RSI. The ANOVA resulted in the following effects. Two main effects were found. First, there was a main effect of current congruency, $F(1, 66) = 159.20, p < .001, \eta_p^2 = .707$, because RTs in incongruent trials were slower ($M = 888$ ms) than in congruent trials ($M = 806$ ms). Second, there was a main effect of complexity condition, $F(1, 66) = 32.50, p < .001, \eta_p^2 = .330$, because RTs in the ´6 S-R´ design condition were generally slower ($M = 877$ ms) than in the ´4 S-R´ complexity condition ($M = 816$ ms). Also, there were two significant two-way interactions. First, the interaction between current congruency and previous congruency, $F(1, 66) = 8.82, p = .004, \eta_p^2 = .118$, indicated a CSE with smaller CEs after incongruent ($\Delta = 72$ ms) compared to congruent trials ($\Delta = 95$ ms). Second, the interaction between current congruency and complexity condition, $F(1, 66) = 6.63, p = .012, \eta_p^2 = .091$, indicated that CEs were smaller in the ´6 S-R´ design condition ($\Delta = 71$ ms) than in the ´4 S-R´ design condition ($\Delta = 94$ ms).The three-way interaction was not significant, $BF_{01} = 5.770$.

CSE analysis across RSI level. Regression coefficients in the ´4 S-R´ design did not differ significantly from zero, $t(66) = -1.062, p = .146, d = -.130$, with moderate evidence for the null model $BF_{01} = 4.357$. For the ´6 S-R´ design regression coefficients also did not differ significantly from zero, $t(66) = 1.2744, p = .897, d = .156$, with moderate evidence for the null model $BF_{01} = 3.445$. Regression slopes in the ´4 S-R´ design were significantly less than in the ´6 S-R´ design, $t(66) = -1.871, p = .033, d = -.229$. Bayes analysis reports anecdotal evidence for the null model, $BF_{01} = 1.445$.

Table 4: Mean RTs (in ms), Error Rates (in %), and CSEs for the Three RSI Conditions (Columns) in Experiment 4 and Mean Regression Slopes Predicting CSEs as a Function of the RSI Condition.

	RTs (ms)					Error rates (%)				
RSI:	**1000**	**3000**	**5000**	**7000**	**9000**	**1000**	**3000**	**5000**	**7000**	**9000**
4 S-R										
Con.→ con.	760	742	751	761	775	8.2	6.2	4.9	5.3	5.4
Con.→ inc.	869	839	823	827	840	14.1	7.3	6.5	6.8	5.2
Inc. → con.	778	770	763	776	786	8.9	4.6	4.0	4.6	5.0
Inc. → inc.	858	831	826	831	830	10.6	7.8	6.5	5.3	7.2
CSE	29*	35*	10	12	20*	4.2*	-2.0	-0.8	0.8	-2.4
Regression	$33.97 + (-4.16)xRSI$					$3.1 + (-1.0)xRSI$				
6 S-R										
Con.→ con.	830	829	818	842	841	9.0	5.9	5.8	5.7	5.5
Con.→ inc.	911	889	899	912	908	13.5	9.5	8.4	8.2	7.8
Inc. → con.	853	842	838	861	860	10.0	5.9	5.7	6.4	6.7
Inc. → inc.	914	903	898	892	909	12.3	7.9	7.8	8.2	8.8
CSE	19*	-1	21*	39*	17	2.1	1.5	0.5	0.7	0.3
Regression	$7.91 + 3.68xRSI$					$2.5 + (-0.5)xRSI$				

Note. Congruency sequence effects (CSEs) significantly different from zero (as indicated by t tests against zero) are denoted by *.

Error rates

CSE analysis for the shortest RSI. For mean error rates the ANOVA revealed one main effect of current congruency, $F(1, 66) = 17.995, p < .001, \eta_p^2 = .214$, because participants committed more errors in incongruent ($M = 14.7\%$) than congruent trials ($M = 11.1\%$). Further, there was one two-way interaction between the factors current congruency and previous congruency, $F(1, 66) = 7.82, p = .007, \eta_p^2 = .106$. The three-way interaction was not significant, $BF_{01} = 5.692$.

CSE analysis across RSI level. Regression coefficients for the ´4 S-R´ design differed significantly from zero, $t(66) = -2.103, p = .020, d = -.257$, with Bayes analysis reporting anecdotal evidence the H1 model, $BF_{01} = 0.949$. For the ´6 S-R´ design regression coefficients did not differ significantly from zero, $t(66) = -0.873, p = .193, d = -.179$, with moderate evidence for the null model, $BF_{01} = 5.179$. Regression coefficients did not differ significantly between conditions, $t(66) = -0.772, p = .222, d = -.094$, with moderate evidence for the null model, $BF_{01} = 5.609$.
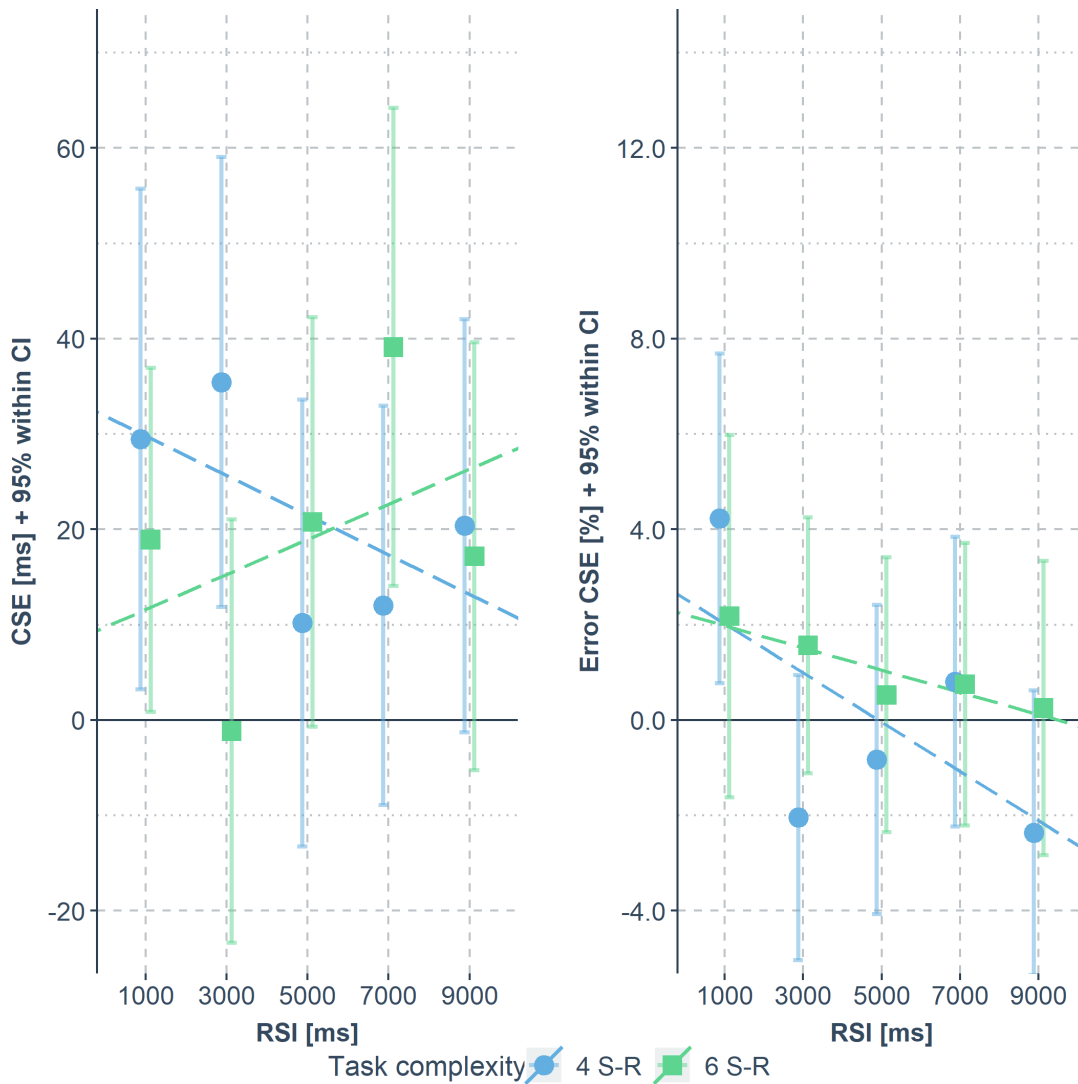
FIGURE 9: CSEs from Experiment 4 in RTs (left panel) and error rates (right panel) with the preceding RSIs on the x-axis. The dashed lines represent aggregated regression models with averaged intercepts and regression coefficients. Error bars indicate the 95% within confidence interval of the CSEs.

Discussion

Experiment 4 had two aims. First, we tested the temporal stability of CSEs in ´confound-minimized´ designs in longer intervals. Results for RTs replicate previous findings, showing that in both complexity conditions CSEs remained stable across RSIs. In addition, experiment 4 extends previous findings by showing stability of CSEs across much longer delays (i.e., 9000 ms) than used in experiments 1-3. For error rates, the picture is more complex. While CSEs decreased in the ´4 S-R´ complexity condition, this effect seems to be driven by a surprisingly large CSE in the shortest RSI condition. Since we did not observe any error CSEs in the three previous experiments using the confound-minimized design, we remain cautious to interpret this observation. Second, we

tested the hypothesis that task complexity (i.e., number of S-R translations) might modulate the time course of the CSE. Here, results were mixed. On the one hand, Null Hypothesis Significance Testing suggests a relatively stronger decay of the CSE for the ´4 S-R´ compared to the ´6 S-R´ condition. On the other hand, Bayesian analysis indicates that more data would be required for a decision between models (i.e., whether slopes differ between conditions). Support for the latter interpretation comes from individual analysis of CSEs slopes in each condition, showing moderate evidence for a temporal stability of CSE over delays for both S-R conditions. The analysis of error rates suggested no condition difference in the time course of the CSE.

General discussion

The aim of this study was to investigate the time-course of the CSE, a prominent behavioral marker to assess cognitive control functions. Previous research found that CSEs show a gradual decrease with increasing time delays between trials, suggesting that the CSE reflects transient control adaptations (Duthoo et al., 2014a; Egner et al., 2010). However, it has been shown that ´repetition´ designs, often used to probe CSEs, reflect influences of both control adaptations and S-R bindings. Against this background, we examined the hypothesis that control adaptations in the CSE, as measured by recently developed ´confound-minimized´ designs are stable across longer temporal delays. To test this, we manipulated the time lag between trials in a ´confound-minimized´ design of the prime-target task. In all four experiments, we found temporally stable CSEs across different time delays between trials (up to 9000 ms).

Another goal of our study was a replication of decreasing CSEs across time in the ´repetition´ design, in order to compare CSEs in both designs directly. While Experiment 2 failed to replicate previous research, Experiment 3 showed moderate to strong evidence for a CSE decrease in the ´repetition´ design. Furthermore, in the latter experiment, direct comparisons between both task designs confirmed that decreasing CSEs in the ´repetition´ design differed from temporally stable CSEs in the ´confound-minimized´ design, providing further evidence that the time-course of the CSE critically depends on the employed design. Experiment 4 tested an alternative explanation for this effect, namely differences in the complexity of S-R translations between designs. Although results replicated and extended the persistent nature of the CSE in the ´confound-minimized´ design, results were more ambiguous regarding a possible influence of S-R complexity on the time-course of the CSEs with weak, but contradictory

evidence from frequentist and Bayesian analysis. In the following, we will discuss these findings and potential implications for our understanding of control processes in the CSE in more detail.

To our knowledge, the present research is the first to show that CSEs reflecting control adaptations independently of S-R bindings are robust against temporal delays. This is potentially relevant for our understanding how control is recruited in CSE paradigms (Botvinick et al., 2001; Gratton et al., 1992; Scherbaum et al., 2012). Control adaptations invariant to temporal delays are in line with the mechanism proposed by the *Expectancy* account (Gratton et al., 1992), which assumes an anticipatory prediction about upcoming congruency level. The findings also fit the cognitive model described in the *Conflict Monitoring Theory* (Botvinick et al., 2001) which proposes a proactive control parameter optimization for upcoming conflict. Finally, these findings could also be interpreted as temporally stable bindings of abstract control parameters (Egner, 2014; Verguts & Notebaert, 2009). However, this conclusion must remain speculative since it remains unclear to which extend control bindings add to the CSE in the used paradigm. Future research could probe the time course of control bindings by examining whether context-transition effects on the CSE (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021) remain invariant across longer temporal delays. On the other hand, results seem incompatible with predictions based in reactive interpretations of the conflict monitoring account. These accounts assume a passive carry-over of control, and hypothesized a gradual decrease in CSEs across time (see Scherbaum et al., 2012). Furthermore, the present results can also not be explained by *S-R binding* accounts, because of the employed ´confound-minimized´ design preventing any S-R repetition effects across consecutive trials.

The observation that CSEs in ´confound-minimized´ design are robust against temporal delays is not incompatible with previous research showing a gradual decrease in CSEs in ´repetition´ designs (Duthoo et al., 2014a; Egner et al., 2010). Indeed, Experiment 3 provided a conceptual replication of these findings. The discrepant results between ´confound-minimized´ and ´repetition´ design for temporal delays could suggest that in the later condition CSEs decrease due to fading of S-R memories. This conjecture is consistent with predictions of the S-R binding account that are based on research showing gradual decay of S-R codes in tasks devoid of response conflict (Frings, 2011; Hommel

& Frings, 2020; Moeller, Pfister, et al., 2016). However, this inference is nec-
essarily indirect because ´repetition´ designs cannot isolate effects of control
adaptations from effects of S-R bindings. Furthermore, closer inspection of the
CSE decrease in the ´repetition´ design in Experiment 3 of the present research
and previous studies suggests differences in ´forgetting´ (i.e., reduction rate)
between tasks. While the present studies observed significant CSEs with RSIs
up to 5000 ms, the studies of Egner et al. (2010) and Duthoo et al. (2014a)
found no CSEs after 2000-3000 ms. Possibly, differences in the tasks employed
affect the time course of the CSE. For example, the prime-target task used
in the present research relies on sequential presentation of prime and target
and therefore requires temporal attention to configure the relative weighting
of prime and target information (Dignath et al., 2021). Mounting evidence
suggests that temporal attention can change the ´integration window´ which
modulates the temporal extend for how long two events are stored within the
same memory trace (Hazeltine et al., 2011). Speculatively, temporal attention
might also modulate retrieval in a similar way, reducing the reduction rate (see
also Altmann & Gray, 2002). To test this, future research could contrast re-
duction rates of the CSE between response-interference tasks that require or
do not require temporal attention. Alternatively, it has been suggested that
control in the prime-target task modulates irrelevant response activation (e.g.
Weissman, 2019) while control in Stroop-like tasks as employed in Egner et al.
(2010) and Duthoo et al. (2014a) modulates relevant target activation (Egner
& Hirsch, 2005). Thus, differences in the reduction rate of CSE might also re-
late to task-specific control mechanisms. Furthermore, it should be pointed out
that the time course of the CSE in both ´repetition´ and confound-minimized
designs could be modulated by other factors than passage of time alone. For
instance, Duthoo et al. (2014a) could show in a second experiment that CSEs
in a ´repetition´ design remained invariant, if the interval duration between
trials was biased towards longer intervals (similar experimental setups are also
commonly used in fMRI studies; see e.g. Egner & Hirsch, 2005; C. Kim et
al., 2014). This suggests that expectations in favor of longer intervals caused
participants to rely on preparatory control instead of short-lived memory strate-
gies. Although the present study presented all temporal intervals equally often
and with equidistance, timing research suggest that temporal intervals are al-
ways coded relatively to other events within a trial (see Gallistel & Gibbon,
2000). Furthermore, experiment 4 of the present research directly addressed
the question whether complexity of S-R translations which could affect working
memory load modulates the time course of the CSE. Although results did not

provide much support for this hypothesis, future research could link working memory and maintenance of cognitive control for the CSE more directly (e.g. Soutschek et al., 2013). Indeed, previous research has already raised the question of the general influence of control and memory strategies in conflict tasks (e.g. Bugg, 2014; U. Mayr et al., 2003; Weissman et al., 2016) and recently has emphasized the importance of the interplay between the mechanisms (e.g. Egner, 2014; Jiang et al., 2015; Jiang et al., 2020), the question of which task features favor the weighting of one or the other mechanism remains unresolved. Relatedly, another possible explanation for a decrease in CSEs in ´repetition´ designs could be an impaired retrieval of control-states. According to the *control binding* account, abstract control parameter become bound to contextual cues like stimuli and responses. Assuming that S-R codes decay over time, the CSE in ´repetition´ designs is potentially reduced for longer delays not because CSEs reflect S-R memories, but rather because decayed stimulus codes which function as retrieval cues are not available and therefore cannot access memories of reactive control adaptation of the previous trial (e.g. Weissman et al., 2016; Egner, 2014; Verguts & Notebaert, 2009; see also e.g. Dignath et al., 2019; Grant et al., 2021). Interestingly, while the CSE in ´repetition´ designs seems to decrease very fast over temporal, unfilled delays (see Experiment 3 of this study; Egner et al., 2010; Duthoo et al., 2014a) comparable effects remain very durable with multiple intervening trials thereby persisting over much longer time frames (Brosowsky & Crump, 2018; Wendt et al., 2006; for similar effects in task-switching see also Waszak et al., 2003; Whitehead et al., 2020, in press). Future research might investigate how conducting an intervening task can help to maintain such bindings.

Conclusion

Four preregistered experiments show that CSEs in ´confound-minimized´ designs, i.e. devoid of S-R repetitions and therefore only driven by control adaptations, are robust against temporal delays. Understanding the time-course of CSEs driven by control adaptations is important for theories of cognitive control which consistently emphasize the need to maintain representation of relevant task goals over longer periods of time (Badre, 2008 for a review) and describe the CSE as a behavioral marker of control adaptations implemented to achieve current action goals (Botvinick et al., 2001; Gratton et al., 1992). However, most control theories lack a clear formalization of temporal aspects of

control implementation. Thus, the current data might help to expand existing theoretical frameworks.

3.2 Study II: Memory for Abstract Control States Does Not Decay with Increasing Retrieval Delays

Moritz Schiltenwolf, Andrea Kiesel, Christian Frings, and David Dignath

Recent studies have suggested that abstract control states (i.e., internal attentional states independent from concrete stimuli and responses) can be stored in episodic memory and retrieved subsequently. However, the duration of such a control state memory remains unclear. Previous research has found a quick and complete decay for stimulus-response bindings after 2000-5000 ms. Here, we tested a possible decay of control state bindings with retrieval delays of 2000, 3000, or 5000 ms. Five preregistered experiments used a confound-minimized prime-target task to measure the congruency sequence effect (CSE) separately for trials in which a nominally irrelevant context feature changed or repeated across trials. Analyses of the individual experiments did not result in conclusive evidence. A mega-analysis integrating the data of all experiments ($N_{total} = 326$) replicated evidence for binding and retrieval of control states, in that larger CSEs were found for context repetition trials. Importantly, Bayesian analysis indicated that this effect was not modulated by the length of retrieval delay. While this finding suggests that bindings of abstract control states can be relatively robust, we also discuss possible limitations of the present research.

Introduction

Human behavior is highly context specific. Seeing the orange lights of roadworks does not bother us as pedestrians, but it immediately calls for more attention when we are driving a car. Theoretical approaches to human action control have acknowledged this by emphasizing the role of memory in adaptive action control (Frings et al., 2020; Henson et al., 2014; Hommel et al., 2001). More specifically, it is assumed that perceived (contextual) stimuli and executed responses are stored in episodic memory in so-called event-files that bind together co-occurring features for a short duration (Hommel, 2004). Repetition of previously encountered features will retrieve other co-occurring features from memory (Colzato et al., 2006, e. g.; Hommel et al., 2014; see also Schumacher & Hazeltine, 2016). This approach has been successful in explaining a wide range of effects, such as action-effect anticipation (Kunde, 2001; Kunde et al.,

2002), stimulus-response translation (Frings et al., 2007; Hommel, 1998), nega-
tive priming (Frings et al., 2015; Rothermund et al., 2005) and task switching
(Kiesel et al., 2010; Koch, Poljac, et al., 2018).

Because situations are often complex and require control over an ever-
changing series of stimuli and possible responses, the question arises as to
whether binding and retrieval is limited to concrete stimulus-response links
or whether it can also account for behavior that relies on abstract representa-
tions (see also Singh et al., 2019). A canonical case of abstraction is cognitive
control, which refers to a set of superordinate functions that allow the mainte-
nance of current goals and task sets independent of specific stimuli or responses
(e.g. Botvinick & Braver, 2015). Cognitive control functions have often been
assessed with response-interference tasks. These tasks manipulate the match
between task-relevant target and task-irrelevant distractor dimensions. For in-
congruent trials, in which the target and distractor indicate different responses,
performance is impaired (longer RTs and more errors) compared to congruent
trials, in which the target and distractor indicate the same response and thereby
facilitate performance.

Interestingly, it has been suggested that the relative weighting of target and
distractor information can be flexibly adapted according to recent experiences
(see e.g. Egner, 2017). For instance, previous incongruent stimuli decrease the
influence of current distractors, whereas previous congruent stimuli increase the
impact of current distractors. This effect, known as the congruency sequence
effect (CSE), has been attributed to dynamic changes in attention (Botvinick
et al., 2001). According to this account, conflict in the previous trial serves as
a learning signal that strengthens relevant and suppresses irrelevant processing
pathways, which reduces the relative impact of conflicting information in the
current trial (but see Lamers & Roelofs, 2011 for evidence that control is driven
by congruent trials). However, in this conflict monitoring account, it remained
unclear how the information about recent conflict experiences, i.e., the learn-
ing signal, is maintained in the time interval between trials. To fill this gap,
a *short-term memory for experienced conflict* was proposed as a maintenance
system for the learning signal (Mansouri et al., 2007; Mansouri et al., 2009).
This idea has been revisited by more recent binding accounts suggesting that
memory stores a snapshot of the attentional state after control exertion (Abra-
hamse et al., 2016; Crump, 2016; Egner, 2014; Schumacher & Hazeltine, 2016).
For instance, the *Binding and Retrieval in Action Control* (BRAC) framework

proposes that, similar to bindings of concrete features such as stimuli and responses, ´instances´ of abstract control parameters (e.g., attentional weights of stimulus and response codes) are integrated into an event-file and can be retrieved under appropriate conditions (Frings et al., 2020). We refer to such internal states as abstract because they modulate the activation of stimuli and responses independently from the concrete perceptual and response features.

This mnemonic control hypothesis has received support from neurophysiological and behavioral studies. For instance, Jiang et al. (2015; see also Jiang et al., 2020) showed that the CSE could be attributed to increased activity in the anterior hippocampus, a region that has been strongly associated with the integration and subsequent retrieval of bindings via pattern completion (Horner et al., 2015; Rolls, 2013). More direct evidence for memory-based control comes from behavioral studies that manipulated retrieval conditions, for instance, by changing the availability of retrieval cues. More specifically, because abstract control states co-occur with the perception of stimuli or the execution of actions in the previous trial, repetition of stimuli or responses in the next trial act as retrieval cues recollecting related control states from memory. Evidence comes from studies that presented a nominally irrelevant context feature that could either repeat or change across trials and reported increased CSEs for context-repetition compared to context-change trials, possibly because context-repetition facilitated retrieval of control states (e.g. Atalay & Inan, 2017; Braem et al., 2014; Kreutzfeldt et al., 2016; Scherbaum et al., 2011; Spapé & Hommel, 2008).

However, in these studies, the lack of experimental control over transitions between specific stimuli and responses posed a challenge that made it difficult to differentiate the effects of control bindings from possible effects of stimulus-response bindings (Hommel et al., 2004). To address this issue, Dignath et al. (2019; see also Grant et al., 2021) implemented a ´confound-minimized´ design with different stimulus and response sets for even and odd trial numbers. This design ensured that stimuli and responses did not repeat across trials. At the same time, a nominally irrelevant context feature (e.g., whether a number was presented as a digit or a word) could change or repeat across trials. Importantly, unlike paradigms in which contingencies between context and congruency levels are learned, context did not provide information about task demands (Crump et al., 2006). They assumed that on each trial the adopted

control state and the displayed context feature would be bound into an event-file (e.g., in an incongruent trial in which the stimuli were displayed as number word, a control state weighting target over distractor information and the number word format become bound in an event-file). Repetition of the context across two trials should result in a retrieval of the previously bound control state. CSEs, serving as markers for the strength of previous control adaptations on current behavior, were larger on context repetition trials than on context change trials. Importantly, these findings could not be attributed to stimulus-response memory, as stimulus and response repetitions were avoided across trials (see Jiménez & Méndez, 2013; Weissman et al., 2014). Additional evidence supporting the effects of control bindings comes from similar studies applying confound-minimized designs to response interference tasks with other contexts such as modality (Grant et al., 2020), task structure (Dignath et al., 2021) or location (Dignath & Kiesel, 2021).

The present research

The present study examined the temporal stability of bound control states. Previous research on binding and retrieval of stimulus-response bindings suggested that event-files decay rather quickly. For instance, Hommel and Frings (2020) found that the aftereffects of stimuli and response codes gradually decreased with longer intertrial intervals (ITIs). This suggests that temporal delays impair retrieval, possibly because event-files that link stimulus-response codes disintegrate over time (Frings, 2011; Frings et al., 2022; Hommel & Colzato, 2004; for response-outcome bindings see Moeller et al., 2016; for neural evidence see Pastötter et al., 2020). The only documented exceptions to such a rapid disintegration are bindings between sequential actions (Moeller & Frings, 2021) and bindings between actions and action effects (Herwig & Waszak, 2012). Both studies showed that ITIs up to 6 sec did not impact the aftereffects of previous trial action codes. To account for their surprising finding, the authors speculated that bindings might serve different functions. Following research on hierarchical action representations (Cooper & Shallice, 2006; see Lashley, 1952), Moeller and Frings (2021) suggested that response-response bindings might enable the formation of complex action representations. For such higher-level representations, temporal stability is relevant because these representations merge temporally distant events. However, at the level of stimulus representations, quick disintegration of stimulus-response bindings seems

more advantageous to prevent interference between individual episodes (Hommel & Frings, 2020). For control bindings it remains unclear which time course is to be expected. Hitherto, only action bindings have been shown to be temporally stable (Herwig & Waszak, 2012; Moeller & Frings, 2021). However, since the confound-minimized design eliminates binding of response codes, one might predict that the context-transition effects on the CSE (c-CSE) becomes smaller with increasing delays, e.g., because representations of perceptual context features decay over time (e.g., Hommel & Frings, 2020). Alternatively, one might speculate that similar to action bindings, control bindings might support complex behavior by balancing in how far attentional settings from previous episodes generalize to new episodes (e.g., Badre et al., 2022). Indeed, a previous study demonstrated that in the confound-minimized design CSEs are robust against time delays of up to 9 sec (Schiltenwolf et al., 2022). In this study features like format, location, or modality were held constant, and thus each trial provided conditions that should facilitate the retrieval of control states from the previous trial. Consequently, temporally robust CSEs in this study might reflect control state retrieval. Based on this perspective, one would assume that c-CSE in the present research – which allow a more direct assessment of control state retrieval – are also unaffected by time delays.

In this study, we aim to examine the temporal durability of abstract control state bindings are. We conducted a series of five preregistered, highly similar experiments in which binding and retrieval of abstract control states could be inferred using a confound-minimized prime-target task. This design eliminates the influences of stimulus-response bindings across sequentially presented trials. Furthermore, we introduced a nominally task-irrelevant context that could either repeat or change across trials. We predicted larger CSEs in context-repetition compared to context-change trials, based on our assumption that control states become bound to the context. Our prediction follows the reasoning that context-repetition trials provide better retrieval conditions than context-change trials, thereby facilitating control state retrieval and leading to stronger control adaptations that are reflected in the size of the CSE. To examine the temporal stability of control bindings, we administered blocks with short and longer ITIs. If control bindings exhibit a time course similar to stimulus-response bindings, we would anticipate smaller c-CSEs in blocks with long ITIs compared to blocks with short ITIs. Conversely, if control bindings are resistant against temporal decay, akin to action bindings, we would expect no difference between c-CSEs in blocks with long and short ITIs. To evaluate

these competing predictions, we used Bayesian inference. Across the experiments, we adjusted three task components to maximize the differences between the critical conditions: First, to put the durability of control state bindings to a stronger test, we increased the ITI durations across experiments (Exp. 1: 2000 ms; Exp. 2 and 3: 3000 ms; Exp. 4 and 5: 5000 ms). Second, Experiment 3 employed an unfilled ITI, based on previous research indicating that bindings decay faster during unfilled intervals (Hommel & Frings, 2020). Finally, in Experiment 5, we added additional context features (Exp. 1-4: Stimulus format; Exp. 5: Stimulus format, stimulus color, and response hand). By enhancing the discriminability between the two varying context levels, we tried to foster the c-CSE measurement.

Experiments 1-5

Methods

Because all five experiments were highly similar, we will describe them together to avoid redundancies. The hypotheses, procedures, outlier criteria, methods, and planned analyses of each experiment were preregistered on the Open Science Framework (OSF, osf.io/k8752/registrations). Raw data, scripts for the experiments, and analyses are available on OSF.

Participants

We collected data from 326 participants (161 female, 152 male, 3 diverse, 10 did not provide gender information; age mean = 29, range: 18-72) in five experiments (N1 = 45, N2 = 60, N3 = 60, N4 = 61, N5 = 100). All participants were right-handed and German-speaking. Experiment 1 was conducted at the lab of the University of Freiburg testing a student sample. All other experiments were online experiments, and participants were recruited via Prolific (Palan & Schitter, 2018). The sample size for Experiment 1 was based on a power analysis using the tool G*Power (Faul et al., 2007). We opted for a test power of 1- = .90, an alpha-error probability of = .05 and an effect size of p2 = .18, which was reported for the c-CSE in the study of Dignath et al. (2019). Sample sizes of Experiments 2-5 all exceeded the calculated sample size of Experiment 1 and were determined using *Sequential Bayes factors* (Schönbrodt et al., 2016)[1].

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[1]For Experiment 2-5, we increased the sample size in batches of 30 participants and tested our main hypothesis under a Bayesian framework. If a decisive Bayes factor (smaller than 1/6 or larger than 6) was observed, we would stop data collection, elsewise we would continue. In Experiment 5, we would start with a minimum sample size of 100 participants to avoid accumulation of misleading evidence in smaller minimum sample sizes as suggested by

TABLE 5: Data exclusion at the participant and trial levels.

Experiment:	1	2	3	4	5
Participant level					
Error rate >75 %	0	0	0	0	1
Error rate deviating >3 SD from sample mean	0	1	1	1	1
Trial level					
First trial of each block	0.8 %	0.8 %	0.8 %	0.8 %	0.4 %
Trials following error trials	7.1 %	5.6 %	5.9 %	5.8 %	7.8 %
Error trials (RT analysis only)	7.2 %	5.6 %	5.9 %	5.7 %	7.8 %
RT >3 SD from participant's sample mean (RT analysis only)	1.3 %	1.4 %	1.4 %	1.4 %	1.1 %

Task and stimuli

The experiment was programmed in JavaScript using the library jsPsych (Leeuw, 2015) and closely followed the paradigm of Dignath et al. (2019). Each trial included the presentation of a fixation cross, a distractor stimulus, a blank, a target stimulus, and a response window (see Fig. 10). The distractor was displayed for 139 ms, followed by a blank screen for 35 ms and the target for 130 ms. In Experiments 1-4, distractors and targets were numbers between '3' and '6'. In Experiment 5, they were numbers between '1' to '4' and '6 to '9'. In congruent trials, the target stimulus was identical to the distractor stimulus but different in incongruent trials. In every trial, the target stimulus was presented slightly smaller than the distractor stimulus. After target presentation, a blank response window followed, which was terminated on response or after a maximum of 1701 ms. Participants were instructed to respond to the target stimulus by pressing the corresponding number button on the keyboard. In Experiments 1-4, participants used only their right hand ('3': index finger, '4': middle finger, '5': ring finger, '6': little finger). In Experiment 5, participants reacted with their left hand to number stimuli in the range from '1' to '4' ('1': little finger, '2': ring finger, '3': middle finger, '4': index finger) and with their right hand to number stimuli in the range from '6 to '9' ('6': index finger, '7':

Schönbrodt et al. (2016). Please note, that while we report in the manuscript the Bayes factors resulting from Bayesian ANOVAs, the stopping rule was applied based on the Bayes factor resulting from Bayesian t-test which was the originally preregistered approach (see "Open Science and Transparency"-statement). The Bayesian ANOVA model uses a different approach to calculate prior distributions than the Bayesian t-test (see Rouder et al. (2012); Rouder et al. (2009)). Therefore, Bayes factors resulting from the Bayesian ANOVA differ (i.e., turned out to be more conservative) from the Bayes factors that were calculated with Bayesian t-tests as the criterion for the stopping rule.

middle finger, '8': ring finger, '9': little finger). If no or an incorrect response was registered, a red screen was displayed as error feedback for 201 ms. Trials were separated by a delay, i.e., the ITI, which was either 'short' or 'long'. In the short ITI condition, the fixation cross was shown for 250 ms, while it was presented for 2000 ms (Experiment 1), 3000 ms (Experiment 2) or 5000 ms (Experiments 4 and 5) in the long ITI condition. In the long ITI condition of Experiment 3, a blank screen was shown for 2750 ms, followed by a fixation cross shown for 250 ms (resulting in a total ITI of 3000 ms).

Additionally, we introduced a context manipulation. Distractor and target stimuli were displayed in either an Arabic digit format (e.g., '3') or as the corresponding German word in capital letters (e.g., 'DREI'). In Experiment 5, we further expanded the context manipulation by introducing additional features of font color and response hand. For instance, one context level could consist of digits, displayed in orange font color requiring participants to respond with their left hand, while the other context level comprise number words, displayed in blue font color, with participants responding with their right hand. Distractor and target would always be presented in the same context.

FIGURE 10: After presentation of a fixation cross, a distractor (1st stimulus, larger size) and a target (2nd stimulus, smaller size) were presented sequentially. Both distractor and target were presented either as a digit or as a word. This manipulation of stimulus format served as a nominally irrelevant context feature that could either repeat (upper panel) or change (lower panel) across consecutive trials (in Experiment 5, font color and response hand were added as context features). Participants were instructed to respond to the target (2nd stimulus) only. The numbers in the word format were presented in German and are translated into English for this figure.

Procedure

After providing informed consent, task instructions were displayed. The participants were instructed to respond as fast and as accurately as possible

and to respond with their right hand only. If the error rate exceeded 40 % in the first ten trials of training, instructions were provided again. If participants failed this accuracy test again, the experiment was terminated.

To avoid confounds of stimulus-response memory (e.g., full or partial stimulus and response repetitions, negative priming or contingency learning), we used a confound-minimized design with two different stimulus-response subsets alternating across (see e.g. Jiménez & Méndez, 2013; Schmidt, 2013; Schmidt & Weissman, 2014; Spinelli et al., 2019) trials so that even trials would use different stimulus-response subsets than odd trials. In each block, each of the responses was paired two times with each level of congruency, previous congruency, context, and previous context, resulting in a total of 128 trials per block. After a training block, participants performed eight experimental blocks. The ITI condition in the first block was randomly chosen, alternating from block to block thereafter. The ITI condition in the first block was randomized per participant. Participants were compensated with ca. 5 £/hr.

Analysis and results

We decided to adjust the preregistered analysis plan by switching from a frequentist to a Bayesian approach (see *Open science and transparency*). Before the test of our main analysis, we successfully validated that the paradigm produced CSEs (see Appendix A.2 for the corresponding analyses; see also table 7).

To test our main hypothesis, we conducted a Bayesian ANOVA with the within factors of context transition [repetition vs change] and ITI duration [short vs long] and participants as random factors with CSE scores as the dependent variables. The CSE score indicates the difference between the congruency effect after previously congruent trials and the congruency effect after previously incongruent trials. It was calculated per participant and condition as $CSE = (meanRT_{con \to inc} - meanRT_{con \to con}) - (meanRT_{inc \to inc} - meanRT_{inc \to con})$. This analysis was repeated with mean error rates as the dependent variable.

With this analysis approach, we tested the hypothesis that the size of c-CSEs is reduced for longer ITIs. Under H1, we expected reduced c-CSEs for longer ITI conditions relative to shorter ITI conditions. Statistically, H1 predicts a two-way interaction between context transition and ITI duration. Bayes factors were calculated as $BF_{10} = \frac{p(data|H1)}{p(data|H0)}$ if $BF_{10} > 1$ and as $BF_{01} = \frac{p(data|H0)}{p(data|H1)}$ if $BF_{10} < 1$. Thus, BF_{10} indicates the likelihood ratio of the probability that the data would

occur under H1 compared to the probability that the data would occur under H0 (e.g., $BF_{10} = 3$ indicates that it is three times as likely to observe the data under the assumption of the H1 model compared to the H0 model), whereas BF_{01} indicates the inverse (e.g., $BF_{01} = 3$ indicates that it is three times as likely to observe the data under the assumption of the H0 model compared to the H1 model). In all analyses, Bayes factors for main effects were calculated against an intercept model for H0 (e.g., for the main effect of context transition: H1 model = CSE~context transition + participant; H0 model = CSE~participant). Bayes factors for interactions were calculated by comparing posterior probabilities for a model including main effects and the interaction term against a model including only main effects but no interaction term (e.g., for the interaction between context transition and ITI duration: H1 model = CSE~context transition + ITI duration + context transition:ITI duration + participant; H0 model = CSE~context transition + ITI duration + participant). We used the standard prior distribution for fixed effects of .5 for all analyses. $BF_{10} < 3$ and $BF_{01} < 3$ are considered indecisive. Error percentages of the Bayes factor estimated with 10,000 iterations of Monte Carlo sampling are reported (a Bayes factor of 10 with an error percentage of 50% can be expected to fluctuate between 5 and 15).

In accordance with our preregistration, we excluded the first trial of each block and all trials following error trials. For RT analysis, we also removed all error trials and trials with RTs deviating more than 3 SD from this participant's conditional mean RT (see Table 5).

The results of the analyses of each individual experiment are described in Table 6.

Discussion experiments 1-5

Experiments 1 to 5 tested whether the c-CSE decreases with increased ITIs. Across the experiments, we varied the duration of the longer ITI (2000-5000 ms), the filling of the ITI (Experiment 3 used an unfilled ITI; all other Experiments showed a fixation cross during ITI), and the type/amount of context features (in Experiments 1-4, the representation of the number stimulus varied; in Experiment 5, the representation of the number stimulus, the color of the number stimulus and the response hand varied). All five experiments remained

TABLE 6: Resulting Bayes factors resulting from the Bayesian ANOVAs conducted on mean RTs and mean error rates of each experiment. Subscript indicates whether it is evidence in favor of the H1 (BF_{10}) or the H0 (BF_{01}). Decisive evidence is printed in bold. In brackets, the Bayes factor error percentage is provided.

Experiment:	1	2	3	4	5
RTs					
Context transition	$BF_{01} = 1.769$ ($\pm1.27\%$)	$BF_{01} = 2.793$ ($\pm1.29\%$)	$\mathbf{BF_{01}=5.559}$ ($\pm\mathbf{2.67\%}$)	$BF_{01} = 1.553$ ($\pm1.32\%$)	$BF_{10} = 2.073$ ($\pm1.28\%$)
ITI duration	$\mathbf{BF_{01}=4.746}$ ($\pm\mathbf{1.84\%}$)	$\mathbf{BF_{10}=4.727}$ ($\pm\mathbf{1.86\%}$)	$\mathbf{BF_{01}=5.603}$ ($\pm\mathbf{1.04\%}$)	$\mathbf{BF_{01}=7.085}$ ($\pm\mathbf{1.76\%}$)	$\mathbf{BF_{01}=6.714}$ ($\pm\mathbf{1.81\%}$)
Two-way interaction	$BF_{01} = 1.789$ ($\pm53.24\%$)	$BF_{01} = 2.121$ ($\pm52.46\%$)	$BF_{10} = 1.421$ ($\pm1.27\%$)	$BF_{01} = 2.340$ ($\pm53.18\%$)	$BF_{10} = 1.241$ ($\pm53.39\%$)
Error rates					
Context transition	$BF_{01} = 2.091$ ($\pm0.83\%$)	$BF_{10} = 1.788$ ($\pm0.83\%$)	$\mathbf{BF_{01}=5.216}$ ($\pm\mathbf{6.65\%}$)	$\mathbf{BF_{01}=7.328}$ ($\pm\mathbf{0.84\%}$)	$\mathbf{BF_{01}=8.703}$ ($\pm\mathbf{0.84\%}$)
ITI duration	$\mathbf{BF_{01}=4.993}$ ($\pm\mathbf{1.04\%}$)	$\mathbf{BF_{01}=3.749}$ ($\pm\mathbf{1.02\%}$)	$\mathbf{BF_{10}=4.021}$ ($\pm\mathbf{1.51\%}$)	$BF_{01} = 2.549$ ($\pm1.03\%$)	$\mathbf{BF_{01}=8.739}$ ($\pm\mathbf{1.03\%}$).
Two-way interaction	$\mathbf{BF_{01}=3.307}$ ($\pm\mathbf{10.33\%}$)	$BF_{10} = 2.778$ ($\pm10.35\%$)	$\mathbf{BF_{01}=5.000}$ ($\pm\mathbf{6.98\%}$)	$\mathbf{BF_{01}=4.672}$ ($\pm\mathbf{10.52\%}$)	$\mathbf{BF_{01}=5.100}$ ($\pm\mathbf{10.63\%}$)

TABLE 7: CSEs in RTs (ms) and error rates (%) and effects of context-transition on the CSE for all five experiments separated.

	CSE in RTs (ms)					**CSE in error rates (%)**				
Experiment:	**1**	**2**	**3**	**4**	**5**	**1**	**2**	**3**	**4**	**5**
Short ITI duration										
Context repetition	48	52	46	33	27	1.2	1.0	2.0	1.4	1.5
Context change	35	41	32	24	23	3.1	1.0	1.7	1.5	0.9
c-CSE	12	11	13	9	4	-2.0	0.0	0.3	0.0	0.6
Long ITI duration										
Context repetition	40	34	32	32	31	2.4	1.8	0.3	0.3	0.9
Context change	34	29	37	23	12	3.1	-1.3	-0.4	0.3	1.0
c-CSE	6	5	-5	9	20	-0.8	3.1	0.7	0.0	-0.1

Note. CSEs were calculated as $(RT_{incongruent} - RT_{congruent})_{N-1\ \text{inc}} - (RT_{incongruent} - RT_{congruent})_{N-1\ \text{con}}$.

undecisive in the test of our main hypothesis. Because all experiments tested the same hypothesis with very similar experimental designs, we decided post hoc to pool the raw data of all experiments (total N = 326) and submit CSE scores to a mega-analysis (also known as *Integrative Data Analysis*: Curran & Hussong, 2009; Eisenhauer, 2021; Hussong et al., 2013) to maximize test power while keeping a more complex data structure than comparable meta-analytical approaches (Sung et al., 2014; Tierney et al., 2015). The mega-analysis tested the hypothesis identical to that tested for each individual experiment, i.e., whether the c-CSE is reduced with longer ITI delays.
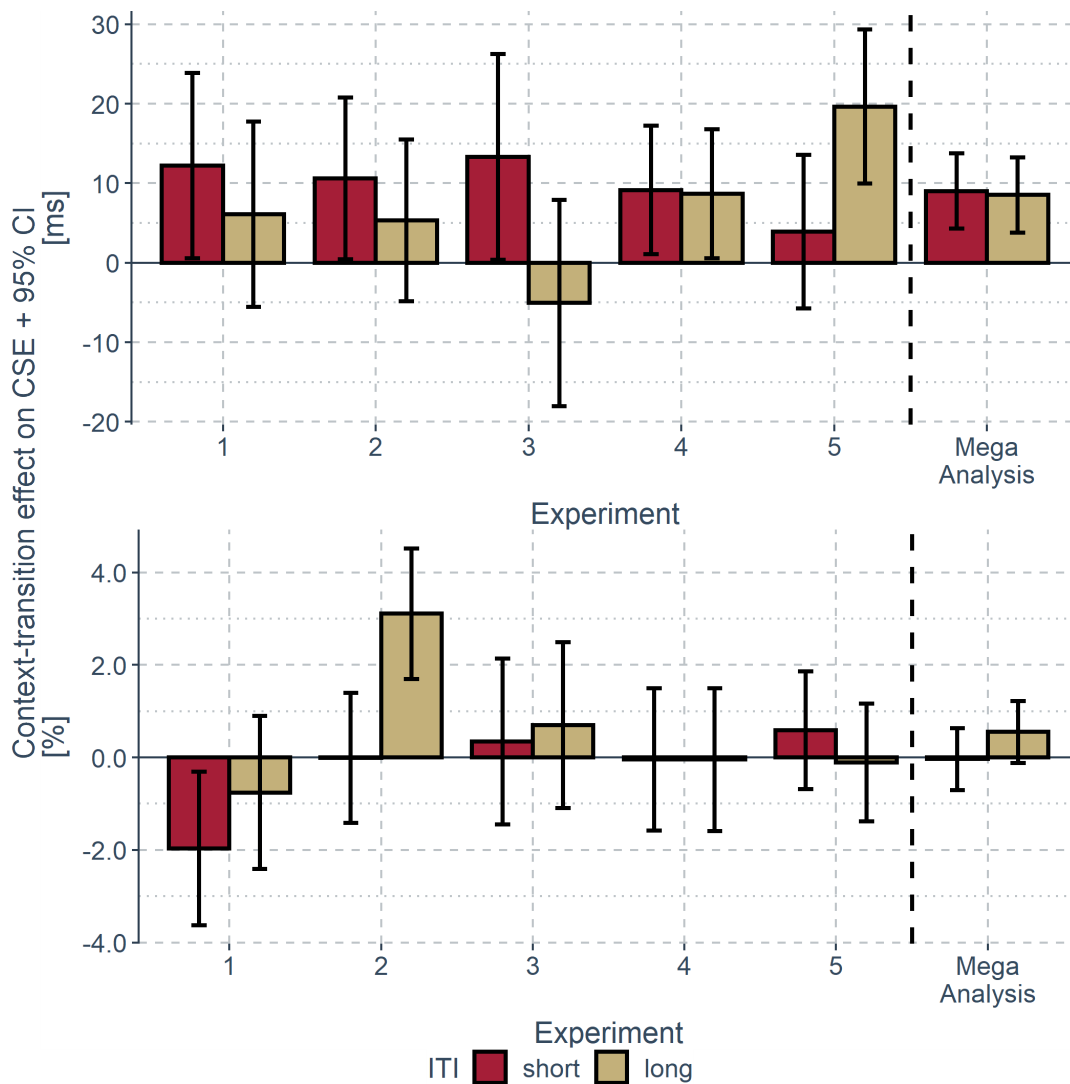

FIGURE 11: Context-transition effects on the CSE (c-CSEs) segmented by ITI condition (color) and experiment (x-axis) with the aggregated c-CSEs on the right side (separated by the dashed line). The upper panel shows the results in RTs, and the lower panel results in error rates. Error bars indicate the 95% confidence interval of paired differences (Baguley, 2012; Cousineau, 2005).

Mega-analysis

Analysis protocol

The mega-analysis repeated the analysis protocol of the previous experiments, with the difference that the data of all five experiments were included and the between-participants factor "experiment" was added. Please note that this additional factor was intended as a control variable and is not designed to be a valid test of differences between experiments because participants were not randomly assigned to a certain experimental condition. For reasons of brevity, we report only the main effect of the factor ´experiment´ and its interaction with the test of the temporal decay of the c-CSE.

Results

According to the preregistrations of the individual analyses, we excluded the first trial of each block (0.7 %) and all trials following error trials (6.5 %). For RT analysis, we also removed all error trials (6.5 %) and trials deviating more than 3 SD from the participants' conditional mean RT (1.3 %). Mean RTs were calculated on an average of 56 observations per condition (with 16 factorial cells: four congruency transitions, two context transitions, and two ITI conditions). A visualization of the results can be found in Fig. 11, while the aggregated CSE scores can be found in Table 8.

The Bayesian ANOVA for CSEs in RTs that tested whether the size of the c-CSE is reduced for longer ITIs yielded the following Bayes factors. First, Bayes factors indicated extreme evidence in favor of a main effect of the experiment factor, $BF_{10} = 101.082(\pm0.59\%)$. Pairwise Bayesian t-tests revealed decisive evidence that CSEs in Experiment 5 ($M = 24$ ms) were smaller compared to CSEs in Experiment 1 ($M = 40$ ms), $BF_{10} = 36.142(\pm0\%)$, and Experiment 2 ($M = 39$ ms), $BF_{10} = 89.464(\pm0\%)$, as well as smaller CSEs in Experiment 2 ($M = 39$ ms) compared to Experiment 4 ($M = 29$ ms), $BF_{10} = 5.546(\pm0\%)$. Second, there was strong evidence for a main effect of context transition, $BF_{10} = 46.280(\pm1.71\%)$, because CSEs were smaller in context change trials ($M = 28$ ms) than in context repetition trials ($M = 36$ ms). Third, Bayes factors remained indecisive regarding the main effect of ITI duration, $BF_{01} = 1.183(\pm0.84\%)$. There was strong evidence against a two-way interaction between context transition and ITI duration representing the test of our main hypothesis, $BF_{01} = 12.330(\pm5.56\%)$. This indicates that the c-CSE did not differ between the short and long ITI conditions. Finally, there was

moderate evidence against a three-way interaction also including the experiment factor, $BF_{01} = 6.974(\pm17.93\%)$.

The same analysis on error rates revealed these Bayes factors. First, there was strong evidence against a main effect of experiment, $BF_{01} = 28.184(\pm0.6\%)$. Second, there was strong evidence against a main effect of context transition, $BF_{01} = 12.551(\pm0.89\%)$. Third, Bayes factors remained indecisive when testing a main effect of ITI duration, $BF_{01} = 1.987(\pm2.73\%)$. Furthermore, there was moderate evidence against the two-way interaction between context transition and ITI duration representing the test of our main hypothesis, $BF_{01} = 8.634(\pm13.58\%)$, indicating that there was no difference in the c-CSE between ITI conditions. Finally, there was strong evidence against a three-way interaction including all factors, $BF_{01} = 24.685(\pm7.36\%)$.

Discussion mega-analysis

To put the hypothesis to the strongest test possible here, we performed a mega-analysis analyzing a substantial sample size of 326 participants. This analysis revealed strong evidence in favor of a c-CSE replicating previous research (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021). Most importantly, the mega-analysis provided strong evidence for the test of our main hypothesis indicating that no effect of ITI duration on the c-CSE was observed.
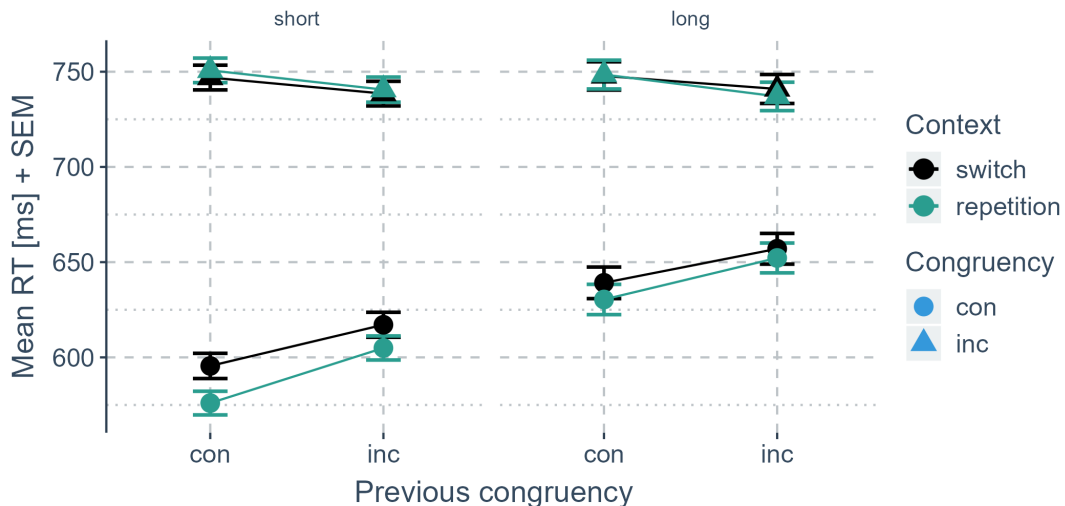

FIGURE 12: CMean reaction times aggregated over all experiments segmented by congruency in the previous trial (x-axis), congruency in the current trial (shape), and context transition (color). Error bars indicate the standard error of the mean for each condition.

TABLE 8: Aggregated CSEs observed in Experiments 1-5 in RTs (ms) and error rates (%) and aggregated effects of context-transition on the CSE

	CSEs in	
	RTs (ms)	**Error rates (%)**
Short ITI duration		
Context repetition	39	1.5
Context change	30	1.5
Context-transition effect	9	0.0
Long ITI duration		
Context repetition	33	1.1
Context change	25	0.5
Context-transition effect	9	0.6

General discussion

The present study aimed to provide a further test of the idea that abstract control parameters are stored in memory. Going beyond previous research, we asked further whether such bindings of control states decay over time or are robust against longer retrieval delays. To probe control states, we measured CSEs in a confound-minimized design of the prime-target task and manipulated whether nominally task-irrelevant context features (in Experiments 1-4, the format of stimulus presentation [word vs. digit]; in Experiment 5, the format of stimulus presentation [word vs. digit], the response hand [left vs. right] and stimulus color [blue vs. orange]) changed or were repeated across consecutive trials. We operationalized retrieval of control states as a benefit (i.e., larger CSEs) for context repetitions compared to context changes. To manipulate the length of retrieval delays, we compared the size of context-transition effects on the CSE using short and longer ITIs. The analyses of the individual experiments' data did not provide decisive evidence when testing our main hypothesis. Furthermore, the c-CSEs observed in these experiments were surprisingly small compared to those reported in previous studies (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021). To obtain maximal test power for the test of our main hypothesis, we decided to integrate the data of all five experiments into a single mega-analysis ($N = 326$).

This mega-analysis, which mimicked the analysis plan of the individual experiments but additionally controlled statistically for potential between-

experiment differences, provided strong evidence that CSEs observed in context-repetition trials are larger than CSEs in context-change trials. Replicating previous research (Dignath et al., 2019; Dignath et al., 2021; Grant et al., 2020), this finding suggests that context-repetitions act as a cue to retrieve abstract control states, supporting the view that internal control parameters are stored in trial-specific event files (Egner, 2014; Frings et al., 2020; Schumacher & Hazeltine, 2016). Second, the Bayesian analysis provided moderate evidence for temporally stable control states for retrieval delays of 2, 3 and 5 seconds.

This temporal stability is in line with a distinction between rapid memory decay for concrete stimulus-response bindings (Frings, 2011; Hommel & Frings, 2020; Pastötter et al., 2020) and a much slower memory decay for more abstract response-response bindings (Moeller & Frings, 2021). For instance, Moeller and Frings (2021) suggested that a quick decay of stimulus-response bindings might be functional because it prevents interference from previous memory episodes. In contrast, more abstract actions require the maintenance of relevant information over longer periods of time, and therefore, such higher-level bindings linking different sub actions would be more efficient if they were temporally more stable. One might speculate that a similar line of reasoning applies to control state binding. Indeed, theoretical models of cognitive control have highlighted the need to maintain abstract control settings over time to ensure adaptive goal-directed behavior (Badre, 2008). Neurophysiological data support such a hierarchical structure (see Badre & D'Esposito, 2009; also Hazy et al., 2007). Control processes based on increasingly abstract rule sets have been located along a caudal to rostral gradient in the prefrontal cortex. Intriguingly, recent data suggest that the same regions (particularly the right middle frontal gyrus) function as a central area for more durable response-response bindings (Geißler et al., 2021). Furthermore, Jiang and colleagues (2015) compared bindings of different abstraction levels (from concrete to abstract: stimulus-response bindings; category-response bindings; control state bindings) and found a distinct neural signature for these types of bindings whereby the allocated brain areas followed a posterior to anterior gradient with increasing abstraction level. Speculatively, bindings that encode more abstract features that control states certainly are might be more robust against temporal decay than bindings reflecting more concrete features. In sum, the present research supports an account differentiating between bindings of abstract relations and concrete features since previous studies reported a fast decay of memory for concrete stimulus-response codes (Hommel & Colzato, 2004; Hommel & Frings, 2020; Frings, 2011; Moeller

et al., 2016; Moeller & Frings, 2017; Pastötter et al., 2020) that was not observed in the present data for memory for abstract control states.

Interestingly, studies in which control states preparing for task switches are paired with unique stimuli (Whitehead et al., 2020) show that such associations can be retrieved even when several minutes have elapsed after the association was formed (Whitehead et al., in press). This is compatible with the present research suggesting that abstract control states are robust against temporal decay. In a similar design, Brosowsky and Crump (2018) showed that in a flanker task, current trial congruency can be influenced by the congruency of a trial that was presented more than 100 trials before if they are both paired with the same unique stimulus. However, they failed to find this effect in a confound-minimized experiment in which the previous and the curent trial have no overlap in the target, distractor and response. This makes it difficult to distinguish whether they observed recall of control states or stimulus-response bindings (Hommel et al., 2004). It remains to be investigated whether the binding and retrieval mechanisms studied in the present research and the more sustained associative learning of control states investigated by Whitehead and colleagues (2020) are independent or similar processes (e.g., Moeller & Frings, 2017, Giesen et al., 2019).

Limitations

A limitation of the present research is the relatively smaller effect sizes of the c-CSE compared to previous findings. For instance, Dignath et al. (2019) observed c-CSEs with an absolute size of 14 ms (Exp. 1) and 24 ms (Exp. 2) and Grant et al. (2021) reported a c-CSE of 32 ms (Exp 1). In contrast, the overall c-CSE in the present research was 9 ms (in both ITI conditions). Consequently, decisive evidence for the test of our main hypothesis, that there is a temporal decay of c-CSEs but also for the to-be modulated effect (c-CSEs) was found only in the extremely high-powered, but not preregistered mega-analysis (but not in the preregistered analyses of the individual experiments). Three methodological factors could account for the smaller effect sizes of the c-CSE in the present study. First, 4 of 5 experiments in the present study were conducted online, while previous research used in-laboratory testing. Although we acknowledge that online testing might induce additional noise, studies that systematically compared in-lab and online testing have found no systematic bias and observed timing accuracy comparable to lab testing conditions (Leeuw & Motz, 2016;

Pinet et al., 2017; Reimers & Stewart, 2015; Semmelmann & Weigelt, 2017). In addition, a direct comparison between Exp. 1 that was conducted in the lab and Exp. 2-5 that were conducted online provided no indication for a difference between in-lab and online studies. Second, the effect sizes of previous research might represent an overly optimistic estimate of the ´true´ effect size. Indeed, research on the so-called ´decline effect´ suggests that effect sizes tend to decrease with increasing years from the first publication of an effect, although the reasons for this decline effect have been debated (see e.g. Lilienfeld & Waldman, 2017). The third factor, which appears most relevant to us, could be due to overall longer delays between trials. Although ITI duration does not seem to have a specific effect on the c-CSE, it could be that overall, longer pauses during trials facilitate mind-wandering, task disengagement and possibly multitasking. Consequently, mind wandering and related off-task behavior during longer waiting periods might have interfered with the encoding and retrieval of control states. For instance, Whitehead et al. (2021) reported impaired encoding of control states in task switching during episodes of mind wandering. Relatedly, Moeller and Frings (2014) found that inattention to retrieval cues impaired retrieval of bindings. Future research could assess these speculations more systematically, for instance, by adding tests of attentiveness to binding and retrieval trials in a comparably strenuous experimental setup.

Conclusion

A mega-analysis integrating the data of five experiments (which provided inconclusive evidence when analyzed individually) found that the c-CSE is robust against temporal delays of multiple seconds. This extends recent accounts such as the BRAC framework, which is concerned with transient memory across subsequent trials (Frings et al., 2014), pointing toward a possible link with associative theories of control that describe a more sustained learning of control (e.g., Abrahamse et al., 2016). However, since the observed c-CSEs were relatively small in the present research, future studies could use alternative paradigms (e.g., Grant et al., 2020) to provide a more detailed picture of how control state bindings play out over time.

Declarations

Ethical approval: The present study fulfils the requirements of the generalized ethical approval by the Ethics Committee for Psychological Research,

University of Tübingen, Germany.

Competing interests The authors declare that they have no conflicts of interest.

Author Contributions All authors contributed to the study concept and design. Programming, testing, and data collection were performed by Moritz Schiltenwolf. Data analysis and interpretation were performed by Moritz Schiltenwolf under the supervision of David Dignath. Moritz Schiltenwolf drafted the article, and Andrea Kiesel, Christian Frings and David Dignath provided critical revisions. All authors approved the final version of the article for submission.

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Availability of data and materials: Data, code, and materials are available on OSF (https://osf.io/k8752/).

Open science and transparency The registrations for all experiments can be found on OSF (https://osf.io/k8752/registrations). We deviated from the preregistered analysis plans for two reasons. First, for experiments 1 and 2, we preregistered a frequentist approach (repeated-measures ANOVA) but decided to switch to a Bayesian framework. This allowed us to compare evidence for a model including an effect of temporal decay on the c-CSE against a null model, and we avoided violations of null hypothesis significance testing assumptions by repeated testing in the mega-analysis. For experiments 3 and 4, we preregistered to test our main hypothesis with a Bayesian dependent measures t-test contrasting difference scores (c-CSE in long ITIs vs. c-CSE in short ITIs). Upon a reviewer's suggestion, we decided to switch from a Bayesian t-test to a Bayesian ANOVA that includes by-participant random intercepts since this more closely followed the preregistered analysis plans for experiments 1 and 2 and allowed us to test for other effects of interest, such as CEs, CSEs, and c-CSEs, in a single analysis. This analysis protocol was preregistered for experiment 5 and applied for all other individual analyses as well as for the mega-analysis. The applied model adhered closely to the originally preregistered analysis plan using the same factors and dependent variables as well as including by-participant random effects. Bayes factors resulting from the Bayesian ANOVA turned out to be more conservative than Bayes factors from the Bayesian t-tests, which were used for the stopping rule in data collection (see footnote on p. 9). The results of the preregistered analysis plan can be found in the online supplement.

3.3   Study III: Contextualized Control: Retrieval or Reset?

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Evidence indicates that trial-by-trial adaptations in cognitive control are reduced when the context changes. The literature offers two explanations for this context-specificity of control: The episodic retrieval account assumes that context repetitions trigger the retrieval of previously implemented control. Alternatively, the attentional reset account suggests that contextual changes disrupt the maintenance of control processes. We conducted three experiments to contrast these accounts and their ability to explain context-specific control adaptation effects. For this purpose, we employed a prime-probe task where the stimuli were presented in one of three formats, which served as context. By manipulating the trial order such that control-inducing and control-probing trials were interspersed with a trial presented in a different context, we derived distinct predictions for the two accounts. According to the episodic retrieval account, control adaptation effects should be larger if the control-inducing and the control-probing trials were displayed in the same context (ABA context sequences) than if the context changes (CBA), while the attentional reset account does not predict differences between ABA and CBA context sequences. Across all three experiments, Bayesian analyses provided decisive evidence against an influence of context transitions on behavioral measures of control adaptations. These findings lend support to the attentional reset account, suggesting that context may act as a signal for when to sustain or discontinue control adaptations. However, we observed no evidence for the episodic retrieval of control.

Introduction

Different situations call for different strategies, whereas currently employed strategies are likely to succeed again if our environment remains unchanged. Cognitive control, i. e. the cognitive functions that coordinate, shield, and regulate the subordinate processes (Miller, 2000) often reflect this simple rule. Prior research showed adaptive changes in control if the context remains constant, whereas no or less evidence for adaptive changes was observed if the context changes (e. g., Dignath et al., 2019; Spapé & Hommel, 2008). Two different accounts have been proposed to explain these findings. Either encountering the same context retrieves a control state previously stored in memory, or

alternatively, changes in the context disturb the maintenance of control states. The present research tests these accounts against each other.

In psychological research, adaptive changes in cognitive control are often probed with response-interference tasks (Braem et al., 2019). These tasks have in common that participants must respond to a task-relevant target while being exposed to a task-irrelevant distractor. In congruent trials, the distractor indicates the same response as the target, whereas in incongruent trials the distractor indicates a different response than the target which makes responding slower and more error prone. Notably, congruency in the previous trial also influences performance in the subsequent trial. More specifically, congruency effects in the current trial are reduced if they follow incongruent compared to congruent previous trials. This data pattern is often referred to as the *Congruency Sequence Effect* (CSE; for reviews see Duthoo et al., 2014b; Egner, 2007, 2017). Theories ascribe the CSE to control adaptations induced by the congruency level of a previous trial that modulate the attentional weights between distractor and target in the current trial (e.g. Botvinick et al., 2001; Gratton et al., 1992; Scherbaum et al., 2012).

Mounting evidence suggests that the CSE is sensitive to changes in the environment to which we will refer to as *context*. Here, we define a context as a set of task features (e.g., additionally present stimulus features) that are uncorrelated with task demands (e.g., the congruency of the current trial).This definition differs from research in which the context, for instance, predicts the congruency level of the current trial or block (Crump et al., 2006). Therefore, the present research focuses on contexts that are nominally irrelevant to the task at hand. In the following, we will review related studies that have observed modulations of the CSE through various context types. Here, the critical comparison is between CSEs when the context repeats across two consecutive trials, and CSEs when the context changes. A typical observation is a reduced or absent CSE for context changes compared to context repetitions (e.g., Dignath et al., 2019; Spapé & Hommel, 2008). We will refer to this effect as the context-specific CSE.

Contexts can be described on different levels ranging from specific stimulus features to more general aspects of the task. In the following, we review studies in which task features were manipulated that could be termed context according to our definition. For instance, Spapé and Hommel (2008) presented words spoken by either a male, a female or the participants' recorded voice as auditory

distractors in a response-interference task. In this study the voice would represent the context. They compared trials depending on context transition, that is whether the voice of the previous trial was repeated or changed across trials. CSEs were found in context repetition sequences, but not in context change sequences. Similarly, studies that manipulated the format of visual stimuli reported reduced CSEs for format changes compared to repetitions (Dignath et al., 2019; Grant et al., 2021). No CSE was observed if the stimulus modality switched between auditory and visual (Grant et al., 2020; Hazeltine et al., 2011; Kreutzfeldt et al., 2016; Yang et al., 2017). Similarly, upon changes in the frequency with which target and distractor flickered (Scherbaum et al., 2011), in the location at which the stimuli appeared (Dignath & Kiesel, 2021; Yang et al., 2021), or in visual features of a cue indicating which stimuli are task-relevant (Braem et al., 2014) the CSE disappeared. Changing the task structure by adding or removing a secondary task (Fischer et al., 2010), or changing the temporal order of target and distractor (Dignath et al., 2021) also led to conditions in which no CSE was observed. Finally, it was shown that CSEs would diminish, if the task itself changes (Akçay & Hazeltine, 2011; Funes et al., 2010), even if the source of conflict was identical (Kiesel et al., 2006).

Two theoretical explanations for the context-specific CSE have been proposed: First, *episodic retrieval* accounts assume that all task features experienced during a trial are bound together and re-encountering one of these features acts as a retrieval cue, recollecting all other bound features (Frings et al., 2020; Hommel et al., 2001). Recently, the concept of binding and retrieval has been applied not only to concrete stimuli and responses, but also to abstract mental states (Egner, 2014; Verguts & Notebaert, 2009). For instance, Egner suggested that control states, reflecting abstract attentional weighting of target and distractor dimensions, become bound together with the currently present context. Repeating the context in the subsequent trial would, in return, lead to the retrieval of the bound control state. As a result, this should facilitate adaptive control, leading to larger CSEs for context repetitions compared to changes. Conversely, changing the context reduces retrieval cues and therefore is accompanied by reduced CSEs. In sum, retrieval accounts explain context-specific CSEs by stronger CSEs for context repetitions due to retrieval of control states.

Second, the *attentional reset* account (Kreutzfeldt et al., 2016) assumes that control states are maintained across trials (as indexed by a CSE), but changes

of context "disrupt" the maintenance of control states. Hence, CSEs are reduced for context changes (Kreutzfeldt et al., 2016). This account was recently specified by Grant et al. (2020) who argued that not every context change disrupts control states, but only if context conditions do not overlap in varying task features (see also Grant et al., 2021; Hazeltine et al., 2011).

The commonly used paradigms to measure context-specific CSEs analyze sequences of two consecutive trials either being displayed in the same or different context condition and therefore cannot dissociate between the episodic retrieval account and the attentional reset account. However, different predictions for these accounts can be derived when an additional trial (N-1 trial) is added between the trial in which the control state is probed (N trial) and the trial that has induced the control state (N-2 trial). Importantly, the context of the N-1 trial must always differ from the context of the N-2 and the N trial (N-2→N-1 and N-1→N context changes), but the context in the N trial is either the same or a different context as the context in the N-2 trial. This results in ABA (N-2→N context repetition) and CBA (N-2→N context change) context sequences (see Fig. 13). In such a design, the effect of interest is the control adaptation effect from N-2 to N, that is, the *N-2→N CSE*. Previous studies have shown that effects of control adaptations can be observed across multiple trials (see e. g., Aben et al., 2017; Horga et al., 2011; Jiménez & Méndez, 2013) and extended time periods (Schiltenwolf et al., 2022). Based on the two competing accounts, we derived the following predictions: According to the episodic retrieval account, a control state is bound to the context in trial N-2. Repeating the same context in trial N should allow the retrieval of the N-2 control state, whereas changing the context in trial N does not allow for retrieval. Under this framework, N-2→N CSEs should be larger on N-2→N context repetitions compared to N-2→N context changes. In contrast, the attentional reset account predicts similar N-2→N CSEs for N-2→N context repetitions and context changes, because the context always changes between consecutive trials (N-2→N1 and N-1→N) and hence should similarly disrupt the maintenance of control states from N-2→N irrespective of the N-2→N context transition.

To test the effect of N-2→N context relation on the N-2→N CSE, we used a prime-probe task (e.g., Kunde & Wühr, 2006) in which number stimuli could be displayed in one of three different formats representing the context (digits, number word, or dice symbols). As described above, format order was determined such that only ABA and CBA context sequences were displayed allowing us

to contrast the episodic retrieval account against the attentional reset account. In Experiment 1, the congruency levels of N-2, N-1, and N were orthogonally balanced. Although both accounts made no predictions for the congruency level of the N-1 trial, it was included as a statistical control variable. In Experiments 2 and 3, we used the identical task design but controlled for response conflict in N-1 by setting it to be always congruent. Additionally, Experiment 3 used a stimulus to key mapping that guaranteed that people used different hands in sequential trials but identical hands in N-2 and N to control for potential effects of hand transitions.
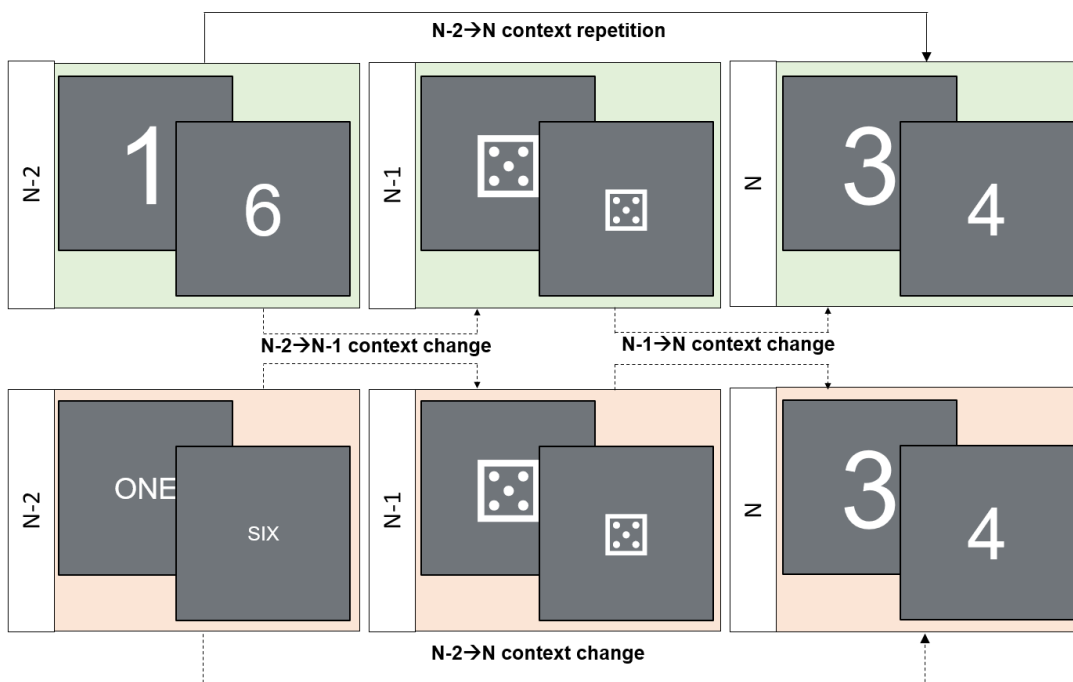

FIGURE 13: The figure depicts two exemplary sequences of three trials. The number format represented the context in this study. In subsequent trials (N-2→N1 and N-1→N) the context never repeated, but from N-2→N the context could repeat (green, upper row) or change (red, lower row). The episodic retrieval account assumes that in N-2 the induced control state becomes bound to the context and repeating the context in N retrieves the control state from memory resulting in larger N-2→N CSEs than on N-2→N context changes. The attentional reset account assumes that the context changes disrupt the currently maintained control state and therefore predicts no influence of N-2→N context transitions on the N-2→N CSE, because disruption is similar for both N-2→N context repetitions and changes

General method

Pre-registrations for all experiments including the hypotheses, procedures, outlier criteria, methods, and planned analyses can be found on the Open Science Framework (https://osf.io/ze9tc/). Raw data, scripts for the experiments, and analyses can be found in the same repository.

Task, Stimuli, and Procedure

The experiments were run on the private devices of the participants using jsPsych (Leeuw, 2015) to build a JavaScript based experiment that could be run on all conventional internet browsers. A minimum display resolution of 1280x700 px was required for experiment participation.

In each trial, sequences of two number stimuli were presented (see Fig. 13). The participants' task was to react to the second of the displayed number stimulus by pressing the respective response button on their keyboard (see the *Experiment-specific methods*-sections). Depending on trial congruency, the first and the second number stimulus (distractor and target) were either identical or different. To manipulate context, the numbers were displayed either as digit, number word or dice symbol. Within a trial the distractor and target were presented in the same context. The distractor was displayed slightly larger than the target. If participants responded incorrectly or outside the response window negative feedback was displayed. Display duration of the trial elements were as follows. Distractor: 139 ms; blank: 35 ms; target 139 ms; response window: max. 1701 ms.

Before the experiment, an algorithm determined trial sequences for all training and experimental blocks according to the following constraints: First, for each block trials were balanced in terms of the congruency levels in N, N-1, and N-2, the context in N and the required response in N. Second, the stimuli were divided in subsets that alternated across three consecutive trials. Such an experimental design avoids stimulus and/or response repetitions (e.g. Schmidt & Weissman, 2014) within three successive trials, i.e. in N-2, N-1 and N never shared the same presented stimulus or required response. Finally, in consecutive trials the context always changed (N-2→N-1 and N-1→N), but from N-2→N the context could either repeat or change.

At the beginning of the experiment, participants provided informed consent, before receiving instructions and performing a training block. Participants who failed to score at least 6 correct responses in the first 10 training trials would have to restart with reading the instructions. If they failed this attention check again, the experiment was aborted. All participants received monetary compensation.

Participants

All participants were recruited via Prolific (Palan & Schitter, 2018) and passed the recruitment filter (English as their first language, and age between 18 and 40 years). The sample size was determined using a sequential Bayesian testing approach (Schönbrodt et al., 2017). We started with an initial participant sample (Exp.1: 100; Exp. 2 and 3: 40) and planned to increase the sample in several batches (Exp. 1: 30 participants per Batch; Exp. 2 and 3: 40) until either i.) analysis results for all hypotheses (CE, N-1→N CSE, N-2→N CSE, context-modulation N-2→N CSE) were unambiguous, i. e. $BF_{10} < 1/3$ or $BF_{10} > 3$ with a default Prior, or ii.) a sample size maximum (Exp.1: 250; Exp. 2 and 3: 120) was reached. In all three experiments, we observed unambiguous evidence after analyzing the data of the initial participant sample.

For each experiment, we applied the following pre-registered exclusion criteria on a participant level (participants who will be completely excluded from the analysis): First, participants who reported that they did not follow the instructions were not considered in the analyses. Second, participants were excluded from all analyses when they committed errors or response omissions in more than 30% of all trials. On a trial level (individual trials that were excluded but the data of participants remained for the analysis), the following data was excluded: First, for the RT analyses, only trials with correct responses and with correct responses for the corresponding reference trials (N-1 and N-2) were used. Second, trials with exceptionally high or low RTs, that is, trials with RTs above/below 3SD from the individual cell mean were discarded.

Analysis

According to the preregistration protocol, we analyzed reaction times (RTs) from all experiments using Bayesian generalized linear mixed models (BGLMM) as implemented in the R package brms (Bürkner, 2017), because the Bayesian

framework allows us to compare the observed evidence for the retrieval account (predicting larger N-2→N CSE in N-2→N context repetitions compared to changes) and the attentional reset account (predicting no effect of N-2→N context transitions on N-2→N CSEs). Participants' RTs on each trial were analyzed using the fixed factors congruency in N (congruent vs incongruent), congruency in trial N-1 (congruent vs incongruent), congruency in trial N-2 (congruent vs incongruent), N-2→N context transition (repetition vs change), and all corresponding interactions. BGLMMs allowed us to account for the specific distribution of RTs typically observed in speeded response tasks where RTs are usually right-skewed, strictly positive, and greater than the minimum time required to encode a stimulus (i.e., the shift of the distribution; see Rouder, 2005). To account for this, we modeled RTs as a *shifted log-normal distribution*. This distribution has three parameters: *mu* (the mean of log-RTs), *sigma* (the standard deviation of log-RTs), and *theta* (the shift). We allowed the mu parameter of that distribution to vary between conditions while keeping sigma and theta fixed across conditions. The BGLMMs always included the maximal random effect structure justified by the experimental design, that is a random intercept for participants and by-participant random slopes for all fixed effects including their and their interaction. We also estimated correlations among random effects. Please note, that we diverged slightly from our pre-registration: First, in Experiment 1, we intended to test the most suitable random effect for our model by fitting a frequentist model with the most complex random effects structure using the lme4 package that corresponds to the brms model (we did that to save computation time). However, most of the models including any random slope using that procedure did not converge which is typical for more complex frequentists modeling fitting techniques. Because that problem does not occur as often using Bayesian model fitting, we opted for the full Bayesian model directly. Second, we intended to fit a simple LMM with a Gaussian-link function on the *logarithmized* RTs. However, a *shifted-log normal* model is a more appropriate model to account for the skewed distribution of RTs that we observed in our data (Rouder et al., 2005). Nevertheless, we fitted all models using the pre-registered models as well and found that this did not affect our results. Hence, we will report the more appropriate models here.

We used informed priors based on several experiments run by the authors with a very similar task design and similar effects of interest. For the intercept we used an *informed Gaussian prior* with a mean of 6.1 (equals an intercept of 645ms - the shift) and a SD of 1.5. For the *ndt* (shift) parameter we used

an informed Gaussian prior with a mean of 5.3 which equals a shift of 200ms and a SD of 0.5. For all other regression coefficients (population-level/fixed effects) and for the sigma parameter (*SD* of individual RTs), we used moderately informative Gaussian priors with a SD of 0.2 and a mean of 0. For random effects (*SDs* of group-level effects), we used non-negative moderately informative Gaussian priors with a *SD* of 0.4 and a mean of 0. We used completely non-informative *LKJ* priors (shape parameter 1) for the correlations of random effects. All categorical predictors were coded as sum-to-zero contrasts. Please note that we ran all models a second time to check if we could replicate findings when using the pre-registered gaussian model with default priors. As reported in the results section, we were able to replicate all findings using this prior sensitivity check.

We estimated the posteriors by sampling parameter values using the *No-U-Turn Sampler* (NUTS, an extension of the Hamilton Monte Carlo sampling method) as implemented in Stan (Carpenter et al., 2017). We sampled generations through four independent Markov chains with 10,000 iterations each (2000 warm up, thus a total of 32,000 post-warmup samples). To investigate convergence, we inspected *Rhat values* (ratio of between-chain variance to within-chain variance) which were all 1.01. Additionally, we validated the models with posterior predictive checks (PPC) by sampling from the estimated parameters' posterior distributions. Then, we overlayed the simulated distributions over the observed data distribution. For all models, the posterior predictive distributions were qualitatively indistinguishable from the observed distributions.

We tested all our hypotheses within a Bayesian framework and calculated Bayes Factors (BFs) to estimate the strength of support for the presence (BF_{10}) or absence (BF_{01}) of an effect using the Savage-Dickey density method. Specifically, we compared the prior and posterior probability densities of a factor/effect by obtaining the full posterior of an estimate and comparing it to the prior density. The BF reflects the resulting ratio of prior to posterior probability for an effect at a given constraint, here at zero to test for evidence for or against an effect. We considered a BF larger than 3 as unambiguous evidence for one hypothesis over the other. To give an example, a BF_{10} of 3 would indicate that the data are 3 times more likely to occur under the alternative hypothesis than under the null hypothesis.

Please note, that we initially intended and pre-registered to calculate BFs

using bridge-sampling. However, given recent debates concerning the instability of BFs obtained using that method (Schad et al., 2022), in combination with increased computing time to fit two competing models for each hypothesis, we decided to use the Savage-Dickey density method instead. However, we calculated the BFs using the pre-registered method for the most relevant factors in this study (e.g., evidence for the absence of a three-way interaction) for all experiments and obtained very similar results as compared to the Savage-Dickey density method.

Experiment 1

The aim of Experiment 1 was to test whether effects of context transitions on the CSE are the result of episodic retrieval of control states or of attentional reset. For this purpose, we used a prime-probe task and presented the stimuli in different contexts (see e.g., Dignath et al., 2019). Importantly, contexts changed across sequential trials, but could repeat from N-2 to N. The episodic retrieval account predicts a larger N-2→N CSE on N-2→N context repetitions than on N-2→N context changes, while the attentional reset account predicts no influence of N-2→N context transitions on the N-2→N CSE.

Experiment-specific methods

Data of 102 participants were collected (parameters for sequential testing were initial sample size: 100; step size: 30; maximum sample size: 250). Due to an error rate of more than 30 % 15 participants were excluded. Data sets of two additional participants were collected because of a technical error. In the final sample, comprised 45 female, 56 male, and one diverse participant. The mean age was 28 years.

The stimulus-key mapping was: '1': 'S' (left ring finger); '2': 'D' (left middle finger); '3': 'F' (left index finger); '4': 'J' (right index finger); '5': 'K' (right middle finger); '6': 'L' (right ring finger). This mapping was independent of the context in which the stimulus was displayed. These mappings were divided in three subsets based on homologous finger pairs. The order across trials determining which subset was used for the current trial was ring finger set → middle finger set → index finger set → ring finger set → […].

Participants performed six experimental blocks each containing 146 trials.

Results

Mean RTs and accuracy rates per condition can be found in Table 9.

To test our hypotheses, we first tested whether we would observe evidence for a four-way interaction between all factors of interest (N congruency, N-1 congruency, N-2 congruency, and N-2→N context transition), to control for potential influences of N-1 congruency on the postulated effects. Indeed, we found that a model including a four-way interaction was superior to a model without that interaction term ($BF_{10} = 3.9x10^7$). Please note, that we did not postulate any specific hypotheses for this interaction, so we cannot provide a hypothesis guided interpretation for that four-way interaction. Nevertheless, we controlled statistically for the effect of the four-way interaction by keeping the four-way interaction term in all subsequent model comparisons.

As expected, we observed a congruency effect (CE), that is RTs were faster in congruent than in incongruent trials ($BF_{10} = 7.9x10^{15}$). The magnitude of the CE was reduced when N-1 was incongruent as compared to when N-1 was congruent ($BF_{10} = 6.3x10^7$), reflecting a N-1→N CSE. Furthermore, the magnitude of the CE was smaller when N-2 was incongruent than congruent ($BF_{10} = 8.0x10^6$), reflecting a N-2→N CSE. However, the size of the N-2→N CSE was similar for N-2→N context repetitions as compared to N-2→N context changes ($BF_{10} = 0.006$; $BF_{01} = 166.7$). That is, we did not find evidence for the hypothesis that bound control states can be retrieved (episodic retrieval account) upon context repetitions (see Fig. 14). We could replicate that finding when using the pre-registered gaussian model with default priors ($BF_{01} = 3333.3$).
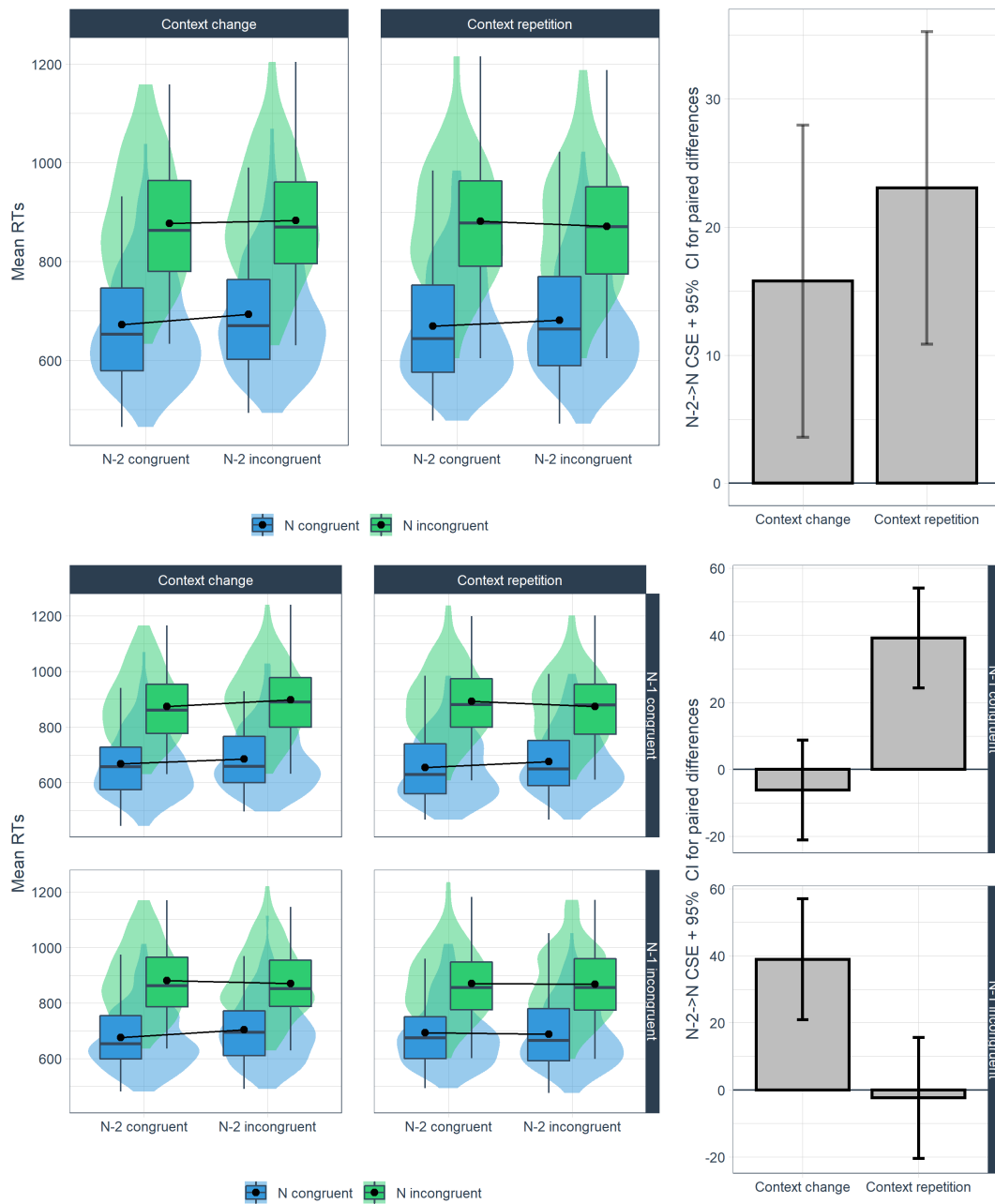
FIGURE 14: Results of Exp. 1. The panels on the left side show mean RTs (y-axis; error bars indicate the standard error of the mean for each condition) in dependency of N-2 congruency (x-axis), N congruency (color), and N-2→N context transition (subpanels). Error bars indicate the standard error of the mean of each condition. The panels on the right side show the same data as the relevant contrast, i. e., the N-2→N CSE in dependency of N-2→N context transitions (x-axis). Error bars indicate the 95% confidence interval for the paired differences. Finally, the top row shows the overall results, whereas the two lower rows show the results in dependency of N-1 congruency.

Discussion

In Experiment 1, we aimed to test whether effects of context transitions on the CSE as observed in previous studies (e.g. Dignath et al., 2019; Grant et al., 2020) are the result of the retrieval of control states bound to the context or the result of attentional reset when context changes. For this purpose, we presented the stimuli in a prime-probe flanker task in one of three display formats that represented the context. Importantly, sequential trials (N-2→N-1 and N-1→N) never shared the same context, but from N-2→N the context could repeat or change.

The results reproduced the behavioral effects commonly observed in response interference tasks such as a CE and N-1→N CSE which validates the comparability to other studies. Further, we observed decisive evidence for an interaction between N-2 and N congruency, i. e. a N-2→N CSE, enabling us to explore further modulations of this effect. Our main hypothesis was tested by comparing evidence for a model including the three-way interaction between N-2→N context transition and the N-2→N CSE with a model excluding this interaction term. In line with the predictions of the attentional reset account, we observed decisive evidence in favor of the model excluding the interaction term.

To control for potential effects of N-1 congruency we included this factor in our model. Surprisingly, we observed decisive evidence in favor of a four-way interaction including all factors (the congruency levels in N-2, N-1, and N, as well as N-2→N context transitions). As shown in Fig. 14 depending on N-1 congruency there were two almost completely opposed effects of N-2→N context transitions on the N-2→N CSE canceling out each other if N-1 congruency was not taken into account. If N-1 trials were congruent, on N-2→N context repetitions the N-2→N CSE was larger than on N-2→N context changes reflecting the results pattern predicted by the episodic retrieval account. However, if N-1 trials were incongruent, we observed the opposite, with larger N-2→N CSEs on N-2→N context changes than on N-2→N context repetitions. Neither the episodic retrieval account nor the attentional reset account predicted the observed results, therefore we decided to hold this factor constant in the next experiments (we come back to discuss the effect of N-1 congruency in the General Discussion).

Experiment 2

The aim of Experiment 2 was to replicate Experiment 1, but to control for the unexpected large influences of the N-1 congruency on the interaction of interest between N-2 and N congruency (N-2→N CSE) and N-2→N context transitions. Because neither the episodic retrieval account nor the attentional reset account provide predictions for the influence of N-1 congruency, we decided to set every second trial (i.e., every N-1 trial) to be congruent. This avoids potential after-effects of experienced response conflict in N-1. The to-be tested hypotheses remained the same as in experiment 1.

Experiment-specific methods

We analyzed a sample of 40 participants (parameters for sequential testing were initial sample size: 40; step size: 40; maximum sample size: 120). No participant was excluded from the analysis. In the final sample were 24 female, and 16 male participants. The mean age was 29 years.

The experiment was identical to the previous Experiment 1 with the difference that every second trial was congruent. The algorithm determining trial order was slightly changed so that also N-2→N context transitions were balanced resulting in five experimental blocks each containing 194 trials.

Results

Mean RTs and accuracy rates per condition can be found in Table 9.

Replicating the findings of Experiment 1, we observed CEs ($BF_{10} = 7.5x10^{31}$) and N-2→N CSE ($BF_{10} = 12.3$). Again, we found evidence against the notion that N-2→N context transitions affected the magnitude of the N-2→N CSE ($BF_{10} = 0.04$; $BF_{01} = 27.03$). We could replicate that finding when using the pre-registered gaussian model with default priors ($BF_{10} = 0.19$; $BF_{01} = 54.2$).

Discussion

Experiment 2 provided a replication of Experiment 1 with the difference that every second trial (i.e., each trial functioning as N-1 trial in the analysis) was fixed to be congruent. In Experiment 1, we observed in trial sequences with congruent N-1 trials larger N-2→N CSEs in N-2→N context repetitions than in context changes, which would be in line with the predictions of the
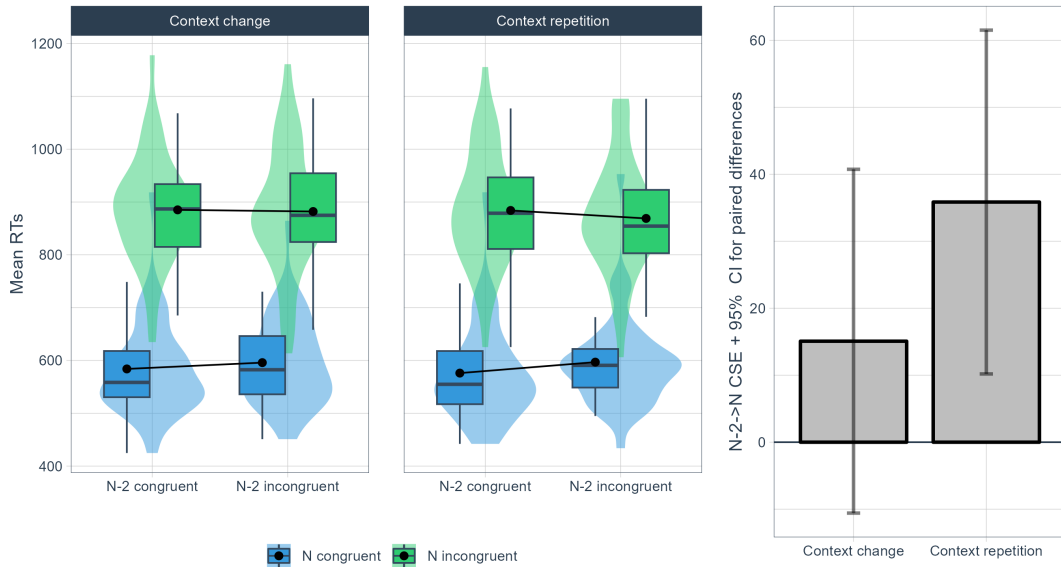
FIGURE 15: Results of Exp. 2. The panels on the left side show mean RTs (y-axis; error bars indicate the standard error of the mean for each condition) in dependency of N-2 congruency (x-axis), N congruency (color), and N-2→N context transition (subpanels). Error bars indicate the standard error of the mean of each condition. The panel on the right side shows the same data as the relevant contrast, i. e., the N-2→N CSE in dependency of N-2→N context transitions (x-axis). Error bars indicate the 95% confidence interval for the paired differences.

episodic retrieval account. This effect was not replicated in Experiment 2. On the contrary, we found decisive evidence against an influence of N-2→N context transitions on the N-2→N CSE which supports the attentional reset account.

A potential confound in the present experiment could be across trial transitions of the response hands. Previous studies have shown that the CSE can be sensitive to response hand switches between the control inducing and the control probing trial (Hazeltine et al., 2011; S. Kim & Cho, 2014; but see Lim & Cho, 2018). Effects of hand transitions could have also influenced the present results. Interestingly, the episodic retrieval account and the attentional reset account could make predictions how response hand transitions would influence performance. Previous binding studies have highlighted that actions itself can retrieve embedded bindings suggesting that actions could provide an hierarchical structure for bindings (Moeller & Frings, 2019, 2021). Following this logic, one could assume that in the present experiment bindings between the provided visual contexts can retrieve the control states bound to them only if also the effector settings are identical, i. e. N-2→N response hand repetitions. However, response hands switches have also been discussed as boundary condition for control adaptations which prohibit their transfer to the next trial (Hazeltine

et al., 2011). Analogous to context switches, hand switches between N-2→N-1 and N-1→N could disrupt control adaptations and thus affect the N-2→N CSE. Unfortunately, the algorithm that was used in this experiment to determine trial order did not balance response hand switches in a way that allows statistical control for this additional factor. For this reason, we conducted a third experiment.

Experiment 3

The aim of experiment 3 was to replicate the findings of Experiment 2 but to control for potential effects of the response hand. Here, we changed the paradigm so that the response hand alternated across trials. In such a design, participants always responded with one hand to the trials that determined the condition for the current trial (i. e., N-2 and N) and with the other hand to the fixed congruent, N-1 trial that was not part of the analysis. Regarding the response hand factor, such a design provides optimal conditions to dissociate the to-be tested accounts. Since N-2 and N always required responses from the same hand, retrieval conditions for embedded context to control bindings should be optimal, while attentional disruptions should also be maximal, since N-2→N-1 and N-1→N require hand switches. Thus, predictions for both accounts should be unaffected by effects of response hand transitions.

Experiment-specific methods

We analyzed a sample of 40 participants (parameters for sequential testing were initial sample size: 40; step size: 40; maximum sample size: 120). Due to an error rate of more than 30 %, 10 participants were excluded. One participant was excluded because after applying trial exclusion criteria, at least one factorial cell had no observations. All excluded participants were replaced. In the final sample were 21 female, and 19 male participants. The mean age was 30 years.

For experiment 3, we used an increased stimulus sample ('1', '2', '3', '4', '6', '7', '8' and '9'). These stimuli were mapped to the matching number keys and participants were instructed to respond with following finger mapping: '1' (left little finger); '2' (left ring finger); '3' (left middle finger); '4' (left index finger); '6' (right index finger); '7' (right middle finger); '8' (right ring finger); '9' (right little finger). The stimulus samples were divided in four sub-samples (sample A: '1', '2'; sample B: '3', '4'; sample C: '6', '7'; sample D: '8', '9').

TABLE 9: Mean RTs (in ms) and error rates (in %) ± standard errors for each trial condition in all experiments and the resulting N-2→N CSE.

| N-2→N context: | | Experiment 1 | | | | Experiment 2 | | | | Experiment 3 | | | |
| | | repetition | | change | | repetition | | change | | repetition | | change | |
		RT	Err.	RT	Err.	RT	Err.	RT	Err.	RT	Err.	RT	Err.
N-2 con.	*N con.*	669 ± 11	6.5 ± 0.4	672 ± 11	5.6 ± 0.4	576 ± 15	2.8 ± 0.4	584 ± 14	3.1 ± 0.5	671 ± 18	5.3 ± 0.5	670 ± 17	5.3 ± 0.5
	N inc.	882 ± 12	11.9 ± 0.8	878 ± 12	12.3 ± 0.8	884 ± 18	10.2 ± 1.3	885 ± 16	9.8 ± 1.2	838 ± 18	13.1 ± 1.5	830 ± 19	14.4 ± 1.5
N-2 inc.	*N con.*	682 ± 11	6.0 ± 0.4	694 ± 11	6.4 ± 0.4	897 ± 14	2.7 ± 0.5	596 ± 13	3.1 ± 0.5	685 ± 17	7.9 ± 0.9	677 ± 17	8.6 ± 0.9
	N inc.	871 ± 12	11.0 ± 0.7	883 ± 12	11.1 ± 0.7	869 ± 17	9.8 ± 1.2	882 ± 19	9.2 ± 1.2	839 ± 18	14.0 ± 1.2	838 ± 20	14.3 ± 1.5
N-2→N CSE		23 ± 4	0.5 ± 0.6	16 ± 4	2.0 ± 0.6	36 ± 10	0.1 ± 1.1	15 ± 11	0.7 ± 1.2	13 ± 8	1.7 ± 1.5	-1 ± 8	3.4 ± 1.2

Note. CSEs were calculated as $(RT_{incongruent} - RT_{congruent})_{N-1\ \text{inc}} - (RT_{incongruent} - RT_{congruent})_{N-1\ \text{con}}$.

Sequential trials were assigned to the stimulus sub-samples following the strict order A→C→B→D→A→ [...]. This order prevents stimulus repetitions across the relevant trials N, N-1, and N-2. Further, across sequential trials the hands to which the required response was mapped switched, and consequently, stimuli of trials N-2 and N required responses from the same hand. As in experiment 2, every second trial (the N-1 trial) was fixed to be congruent.

Given that hands switched across sequential trials, participants responded always with the same hand to the fixed congruent trials and with the other hand to the remaining either congruent or incongruent trials (the N-2/N trials). To control for effects of the effector hand, we counterbalanced across participants with which hand participants responded to the fixed congruent trials.

Results

Mean RTs and accuracy rates per condition can be found in Table 9.

Replicating our first two experiments, in Experiment 3, we again observed a CE ($BF_{10} = 1.3x10^{17}$). However, we did not find evidence for an N-2→N CSE ($BF_{10} = 0.064$, $BF_{01} = 15.6$). Again, we did not observe a larger N-2→N CSE on N-2→N context repetitions as compared to N-2→N context changes ($BF_{10} = 0.027$; $BF_{01} = 37.0$). As in Experiment 2, we could replicate that finding when using the pre-registered gaussian model with default priors ($BF_{01} = 91$).
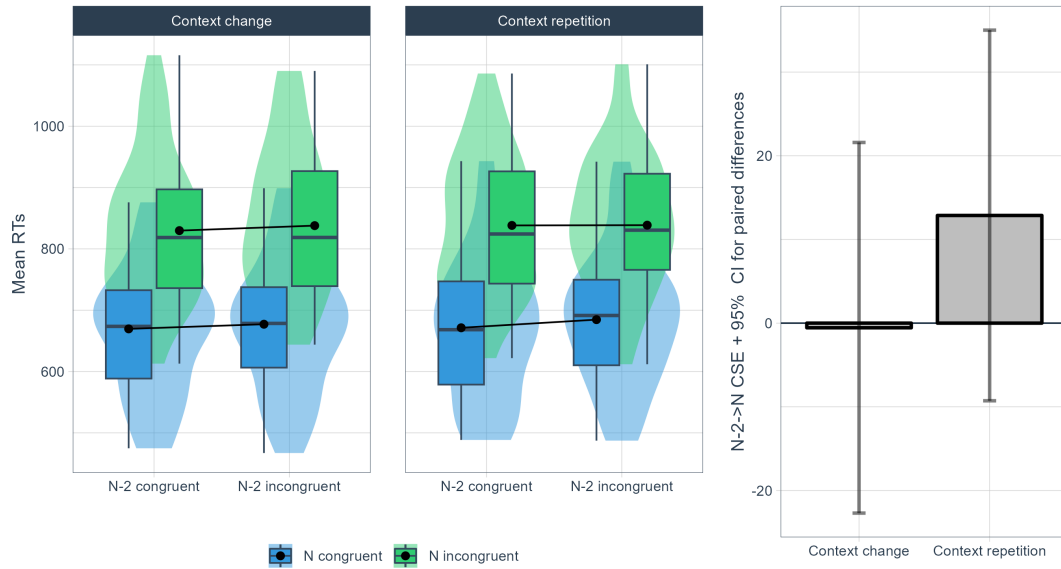
FIGURE 16: Results of Exp. 3. The panels on the left side show mean RTs (y-axis; error bars indicate the standard error of the mean for each condition) in dependency of N-2 congruency (x-axis), N congruency (color), and N-2→N context transition (subpanels). Error bars indicate the standard error of the mean of each condition. The panel on the right side shows the same data as the relevant contrast, i. e., the N-2→N CSE in dependency of N-2→N context transitions (x-axis). Error bars indicate the 95% confidence interval for the paired differences.

Discussion

In Experiment 3, we aimed to replicate the findings of Experiment 2, but to change the paradigm so that participants reacted with one hand to the N-2 and N trials and with the other hand to N-1 trials. This adjustment controlled for influences of response hand transitions, since N-2→N hand repetitions provided optimal retrieval conditions for bindings between the context and control states and sequential hand switches (N-2→N-1 and N-1→N) maximize contextual changes, thus increasing potential effects of attentional disruption. As in Experiment 2, we found decisive evidence against an effect of N-2→N context transitions on the N-2→N CSE which supports the attentional reset account stating that control adaptations induced in N-2 diminished with the sequential contextual changes in N-2→N-1 and N-1→N.

Further, we observed decisive evidence against an overall N-2→N CSE, which was observed in the previous experiments. This post-hoc finding could be interpreted as additional evidence in favor of the attentional reset hypothesis, because independent of context transitions sequential changes in the response hand could already lead to a complete deterioration of the N-2→N CSE.

General discussion

In three experiments, we asked whether the context-specific CSE (e. g., Dignath et al., 2019; Spapé & Hommel, 2008) reflects episodic retrieval of bound control states triggered through context repetitions or rather indicates attentional reset of maintained control states due to context changes. For this purpose, we measured N-2→N CSEs in a prime-probe task with format contexts for each trial. Importantly, we adjusted trial order so that contexts always changed from trial to trial (N-2→N-1 and N-1→N) but could change or repeat from trial N-2 to trial N (N-2→N context repetitions/changes). These specific trial sequences allowed us to dissociate between the episodic retrieval and the attentional reset account, because the two proposed accounts make different predictions for the N-2→N CSE in trial sequences with such context transitions: The episodic retrieval account assumes that in the N-2 trial control states induced by trial congruency become bound to the stimulus context. Independently from the intervening N-1 trial, this account would predict that re-encountering the context of N-2 in the N trial leads to the retrieval of the control states bound to this context, whereas no retrieval is to be expected on N-2→N context changes (Egner, 2014; Frings et al., 2020). This retrieval of control states should be reflected by larger N-2→N CSEs in N-2→N context repetitions than in N-2→N context changes. Alternatively, the attentional reset account assumes that control states are reset if the context in which these states were adopted changes. Therefore, attentional reset should not be affected by the context relation between the N-2 and the N trial, and thus the account would predict no influence of N-2→N context transitions on the N-2→N CSE. To dissociate these two accounts, we used Bayesian generalized mixed models to compare evidence for a model that includes an interaction between N-2→N context repetitions on the N-2→N CSE (as predicted by the episodic retrieval account) with evidence for model excluding this interaction term (as predicted by the attentional reset account). In three experiments, Bayesian analysis revealed decisive evidence against an influence of N-2→N context transitions supporting the attentional reset account over the episodic retrieval account. Notably, these results remained unchanged when Experiments 2 and 3 controlled for influences of response conflict in the intervening N-1 trial, and Experiment 3 controlled for influences of response hand transitions.

Interestingly, further support for the attentional reset account comes from

the post-hoc finding of missing N-2→N CSEs in Experiment 3. In Experiments 1 and 2 Bayesian analysis provides robust evidence for an overall N-2→N CSE, that is, aggregated over N-2→N context repetitions and changes, whereas Experiment 3 showed evidence *against* an N-2→N CSE. The major change in experiment 3 was that the trial order was adjusted to control for hand repetitions. That is, in sequential trials participants had to respond with different hands, whereas in Experiments 1 and 2 the response hand could repeat across trials. The observation that no N-2→N CSE was found in Experiment 3 could be interpreted post-hoc as evidence for an attentional reset triggered by hand switches. Indeed, this possibility was already pointed out in previous research (Hazeltine et al., 2011; S. Kim & Cho, 2014).

A possible alternative explanation for our main finding could be that over time a memory trace for control states decays. While decay can explain reduced memory for stimuli and responses for longer delays (Frings, 2011; Frings et al., 2022; Hommel & Frings, 2020; Moeller, Pfister, et al., 2016), it seems unlikely that decay of control bindings can explain the observed effects in the present study. Previous studies showed that (in contrast to stimulus-response effects) the CSE (Schiltenwolf et al., 2022) and the context-specific CSE (Schiltenwolf et al., 2023) is robust against longer temporal delays. Also, studies have successfully reported N-2→N CSEs (Aben et al., 2017; Dey & Bugg, 2021; Jiménez & Méndez, 2013; S. Kim & Cho, 2014). Furthermore, studies focusing on retrieval in color discrimination tasks or task switching have already presented evidence for action to context binding effects across two trials (Grange et al., 2017; Mocke et al., 2023; Rangel et al., 2023).

Although the observed evidence for the attentional reset account seems compelling, it remains unclear which mechanism drives the attentional reset (Kreutzfeldt et al., 2016). Here, we speculate that perspectives from recent working memory literature can explain attentional reset. Oberauer (2019, p. 13) suggested that "working memory plays a crucial role in controlling attention and action by holding the representations that guide attention and action". Let us assume that transient bindings between contexts and control states are held active in working memory (Oberauer, 2009). Furthermore, we assume that working memory capacity is limited, hence, it must be updated so that irrelevant information can be removed (e. g., Dames & Oberauer, 2022; Oberauer, 2019). If the context changes, previous bindings are deemed irrelevant and consequently removed from working memory. Under this perspective, context acts

as a gating signal, indicating the need to update working memory (Braver & Cohen, 2000; Chiew & Braver, 2017; Ott & Nieder, 2019). In the present study, we reason that participants utilized the stimulus format as a context, because the stimulus format is necessary to process the target information. Changes of context then indicate when to update working memory.

Furthermore, it remains an open question what caused the strong modulating influence of N-1 congruency in Experiment 1 that resulted in the reported four-way interaction which could not be accounted for by episodic retrieval or attentional disruption. Possibly, the observed effect resulted from confounding variables that partially overlapped with the manipulated factors. A candidate for such a confound could be that hand transitions were not controlled for in Experiment 1. Exploratorily, we found that control adaptation effects from N-2→N and from N-2→N were further modulated by several higher order interactions of hand and context transition factors. One could speculate that a mixture of control adaptation in N-1, control reset upon hand switches and bindings between the context and the response features such as the effector hand could result in the reported four-way interaction. However, it should be noted that we cannot conclusively explain this effect. For a better understanding of possible confounds, future research might try to isolate processes that were potentially overlapping, such as the proposed response effector bindings and their interplay with response conflict. For the scope of this study, it is important to emphasize that after controlling for N-1 congruency in Experiments 2 and 3 the behavioral pattern observed in Experiment 1 for N-1 congruent trials (which matched the predictions of the episodic retrieval account) did not replicate, but decisive evidence was observed against an influence of N-2→N context transitions on the N-2→N CSE.

To summarize, this study investigated whether effects of nominally task-irrelevant context transitions on the CSE can be rather explained as episodic retrieval of control states (repeating the context leads to control retrieval) or as attentional disruption (changing the context disrupts adapted control states). In three experiments using prime-probe tasks with varying contexts, Bayesian analysis provided decisive evidence against the episodic retrieval account and thus suggests that changes in contexts disrupt active control states thus reducing observed CSEs. Importantly, even after accounting for the impact of response conflict during the disruption and the impact of hand transitions, this effect remained consistent.

Declarations

3.4   Study IV: Binding of Response-Independent Task Rules

Moritz SCHILTENWOLF, David DIGNATH, and Eliot HAZELTINE

Binding theories claim that features of an episode are bound to each other and can be retrieved once these features are re-encountered. Binding effects have been shown in task switching studies with a strong focus on bindings of observable features such as responses. In this study, we aimed to investigate whether task rules, translating stimulus information into motor output can be bound and subsequently retrieved even if they act independently from specific response codes. To address this question, we utilized a task switching paradigm with varying visual context features. Unlike previous studies, tasks in the present study did not differ in their response options, and sequential response repetitions were eliminated by design. In two experiments, we observed larger task switch costs on trials repeating the context of the previous trial than on context change trials. This suggests that response-independent task rules adopted in the previous trial became bound to the context feature and were retrieved upon re-encountering the context feature in the current trial. The results of this study generalize previous findings indicating that binding processes can include response-independent control to task switching situations.

Introduction

Storing current experiences in memory guides future actions. The interplay between integration of sensorimotor information and subsequent retrieval – a core mechanism driving human behavior – is addressed by binding theories (Frings et al., 2020; Hommel et al., 2001). While studies show that features of stimuli and responses can be rapidly integrated into instances of episodic memory and retrieved (e. g., Rothermund et al., 2005), it remains up for debate whether the same binding mechanism also applies to abstract task rules, i. e., the cognitive representation of rules how to translate the stimulus input into correct motor output (e.g., U. Mayr & Bryck, 2005; Vaidya & Badre, 2022). In this study, we examine whether task rules are bound with visual contexts so that repeating the context allows for subsequent retrieval of these task rules. Importantly, we employed a paradigm under which such effects cannot reflect the retrieval of previously activated responses or responses that are generally linked to a specific task (Oberauer et al., 2013).

When people switch between multiple tasks, goal-based behavior requires appropriate task sets to shield and schedule individual tasks (Rogers & Monsell, 1995). While there are different definitions for the term "task set", many researchers agree that task sets orchestrate the identification of task-relevant stimuli, the selection, and execution of responses (e.g., Vandierendonck et al., 2010; Schneider & Logan, 2014). To study task sets, researchers rely on task switching paradigms in which participants are required to switch flexibly between different sets of rules (i.e., tasks) to produce the appropriate response (for a review see Koch, Poljac, et al., 2018). Examples for such tasks include stimulus classification, arithmetic operations, or spatial operations (U. Mayr & Kliegl, 2000; Allport et al., 1994; Baddeley et al., 2001).

Switching from one task set to another is a costly process, reflected by worse performance on trials involving a different task from the previous trial than on trials repeating the previous task (switch costs). Traditionally, switch costs are attributed to either a reconfiguration process, during which the new task set needs to be implemented (Rogers & Monsell, 1995; Rubinstein et al., 2001) or to interference between residuals of the previously implemented and the new task-set (Allport et al., 1994). However, binding theories (Frings et al., 2020) offer an alternative interpretation. The basic assumption of these theories is that events are transiently encoded through the features of concurrently perceived stimuli, performed actions and produced action-effects (Frings et al., 2020; Hommel et al., 2001; see also Kahneman et al., 1992). Hence, when one of the previously bound features is encountered again, all other features that were linked to the repeated feature are retrieved. As a consequence, a match between bound task features with the current task demands (e.g., by activating the correct response) facilitates performance whereas a feature mismatch requires an updating which impairs performance (e. g., Frings et al., 2015; Rothermund et al., 2005; Foerster et al., 2021; Hommel et al., 2004; Stoet & Hommel, 1999).

Binding perspectives have also inspired recent formalizations of task-sets. For example, Oberauer et al. (2013) postulated that task sets can be described as bindings between stimuli or stimulus categories, corresponding responses, and expected outcomes in working memory. By this account, switch costs are assumed to be the product of interference between currently active bindings and residual activation of outdated bindings, and/or resource intensive memory updating processes. Another account based on episodic encoding, the Parallel Episodic Processing model (Schmidt et al., 2016), assumes that stimuli, task

goals, task decisions, and responses are integrated into memory by an iterative process (Schmidt et al., 2020). This model holds that when both the task cue and the required response repeat, switch costs will be inflated by bindings. This is because the task cue repetition will not only trigger the retrieval of the task rule, but also retrieval of the stimulus and response codes of the previous trial. Therefore, if consecutive trials match on these codes, performance will be facilitated. In contrast, when the task switches, costs can arise from stimulus repetitions because they were bound to different task rules and possibly different responses (Allport & Glenn, 2000; Schmidt & Liefooghe, 2016).

Effects of response bindings on task switching have been tested experimentally by manipulating context features (e. g., Koch, Frings, & Schuch, 2018). In task switching research, context manipulations are often implemented as informative cues for certain task demands. For instance, Crump and Logan (2010) employed the location at which stimuli were presented as an informative context, instructing participants about the likelihood of encountering either the same task as the previous trial or a different task. Hence, context are correlated with task demands by design and findings from these studies show that participants learned such contingencies and retrieved context-appropriate control states (see also Chiu & Egner, 2017; Leboe et al., 2008). However, context can also influence behavior in task switching without being directly linked to specific task demands. In contrast to the aforementioned context-correlation design, situation we have in mind are those in which contexts are orthogonal to the specific task demands (i.e., task demands and context are *not* correlated). Since we want to explore such effects in this study, we will refer in the following to the term *context* as task features that are not informative about current task demands such as whether a specific task or task switch is to be expected or which response is required. Koch, Frings, and Schuch (2018; see also Kandalowski et al., 2020) used the task-cue modality as context. Although context and responses were uncorrelated, they found that response repetition benefits that are usually observed for task repetitions were restricted to context repetitions. This pattern was also observed for other context features such as, visual features (Benini et al., 2022a, 2022b), action effects (Schacherer & Hazeltine, 2022), or language (Benini et al., 2022b). Binding theories explain this by assuming that even task-irrelevant context features are bound with the task-relevant features (Frings & Rothermund, 2017). Trial sequences in which context features change from the previous episode while all other stimulus and

response features repeat yield worse performance than context repetition sequences because features from the old episode unfit for the demands of the current episode might be retrieved or resources must be allocated towards the updating of active bindings (Hommel, 1998; Mocke et al., 2023; Rothermund et al., 2005; Moeller, Frings, & Pfister, 2016).

Previous research investigating how binding affects task switching has focused on binding and retrieval of specific responses. However, this emphasis on the relation between stimuli and responses hinders a possible generalization of binding mechanisms in task switching. Critically, previous research can only account for binding effects in tasks in which stimuli (categories) map to specific responses (for an in detail discourse, see Hazeltine & Schumacher, 2016). This is important because several studies indicate that task sets incorporate task rules. Our use of the term task rule derives from work of Mayr and Bryck (2005) and refers to the translation of stimulus input into motor output on a more abstract level than simple stimulus-response mappings. For instance, U. Mayr and Bryck (2005) introduced a task switching paradigm in which switch costs were observed although task switch/repetition sequences used the exact same stimuli and responses, suggesting that switch costs arise due to task rules that provide an appropriate link between stimuli and responses (see also Waszak et al., 2003, Exp 5). Analysis of neurophysiological and behavioral data of participants performing such a task switching paradigm suggests that only the strength of EEG-correlates representing bindings between stimuli, responses, and task rules predict behavioral binding effects, not those including only stimuli and responses (Kikumoto & Mayr, 2020). Finally, Haynes et al. (2007) used a voluntary task switching paradigm in which the two arithmetic tasks shared the same stimulus and response options. The researchers were able to predict the to-be-performed task during the preparation period from decoded brain activity measured with fMRI. Since these results cannot stem from task-specific stimulus or response code activation, it suggests that task rules can be differentiated on a neural level.

These studies indicate that task sets are include more than specific stimulus to response mappings but also comprise the task rules that control correct stimulus to response translation. In other words, to perform a task it is not sufficient to identify the relevant stimuli and responses; it is also necessary to have the correct task rules active, especially when multiple tasks overlap in the

pool of stimuli and responses relevant to them. However, studies investigating binding effects on task switching have focused on the retrieval of responses (e.g., Benini et al., 2022a; Kandalowski et al., 2020) and thus it remains unclear whether response-independent task rules can be part of bindings (Egner, 2023). In this regard, it is notable that in the related field of conflict adaptation, studies have shown that cognitive states that control attentional weights independently from specific stimulus or response codes can be bound to context features and retrieved upon context repetitions (Dignath et al., 2019; Dignath & Kiesel, 2021; Grant et al., 2021; Spapé & Hommel, 2008; for theoretical perspectives see Egner, 2014, 2017), but whether this applies to task rule binding is unexplored.

Here, we address this question by testing whether response-independent task rules that guide the translation of stimulus input into response output can become bound and retrieved. We use a task switching paradigm similar to U. Mayr and Bryck (2005; see also Kikumoto & Mayr, 2020; Rangel et al., 2023) in which participants perform one of three spatial operation tasks (Fig. 17). A strength of this paradigm is that it controls for the impact of stimulus and response bindings on switch costs (Schmidt & Liefooghe, 2016), since the tasks cannot be distinguished by specific response mappings or sets and response repetitions across trials can be avoided in an intuitive way. Consequently, task switch costs should reflect costs of switching task rules that translate the stimulus setup into an appropriate response.

To assess whether such task rules can be bound and retrieved, as predicted by binding accounts, we presented visual context features (colorized background patterns) that either repeated or changed across trials. We hypothesized that response-independent task rules became bound to the context feature and were retrieved upon the re-occurrence of the context feature. Therefore, we expected larger switch costs on trials where the context repeated from the previous trial than when the context changed, since the retrieval of the task rules that were adopted in the previous trial should be facilitated, which in turn should improve task performance in task repetition trials and impair performance in task switch trials (see Fig. 17). To test this prediction, we conducted three structurally identical experiments. In Experiment 1, we used a trial order resulting in 50% task and context repetitions, while in Experiment 2 the chance for each task and context combination was independent from the previous trial (which equals to

33% task and context repetitions). In Experiment 3, we controlled for stimulus-to-stimulus bindings between the context and the task cues by mapping two task cues to each task and ensuring that task cues never repeat across trials.

Experiment 1

Methods

The hypothesis, procedure, outlier criteria, methods, and planned analysis were preregistered on OSF (https://osf.io/rb73g). Raw data, scripts for the experiments, and analysis are available on OSF.

Participants

We analyzed a sample of 45 participants (11 female, 31 male; mean age: 28; 3 participants provided no demographic information). All participants were recruited on Prolific (Palan & Schitter, 2018) and were in the age range of 18-40 years, had German as first language, and had no issues seeing colors. A pilot study indicated an effect size for task binding of $d_z = 0.968$ which would require a sample size of 14 participants to achieve a test power of 95% with a .5 alpha criterion. Since the pilot study used a different task and context manipulations, we decided to increase the sample size. No participant was excluded from the analysis.

Task and stimuli

The experiment was coded for a browser environment using the JavaScript based library jsPsych (Leeuw, 2015). During the experiment, four black boxes were continuously displayed in a 2x2 grid. One of the boxes was the starting box for the current trial, and the participants were instructed to identify the correct goal box depending on the indicated task rule. The goal box of the current trial always was the starting box for the next trial.

Each trial followed this structure (display duration in parentheses): Fixation cross without context (500 ms), fixation cross and context onset (500 ms), blank (35 ms), task cue (1500 ms or until a response was given). At the beginning of each trial, a fixation cross was presented in the starting box. Upon context onset, the background of all boxes was filled with one of the three colorized context patterns (green chess board, yellow serpentines, blue zigzags) which lasted until the end of the trial. During the blank neither the fixation cross nor

task cue were visible. The task cue was presented superimposed and centrally in the same box as the fixation cross indicating which of the three tasks the participant had to perform in the current trial. The task rules were *clockwise* (correct response is the next box in clockwise direction; indicated by "♥"), *counter-clockwise* (next box in counter-clockwise direction; indicated by "♣"), and *across* (box on the diagonal opposite side; indicated by "★"). Depending on the task, the participants had to decide which of the boxes would be the correct goal box and provide the response via key press (top left box: Key "R" with left middle finger; bottom left box: Key "F" with left index finger; top right box: Key "T" with right middle finger; bottom right box; Key "G" right index finger). Giving a response ended the current trial. Giving no or an incorrect within the stimulus duration was registered as error feedback was presented for 1500 ms (the screen turned red and "WRONG BOX!" on normal trials or "PAY ATTENTION TO COLOR AND SHAPE!" on catch trials was presented in German centrally on the screen). Since the starting box of each trial was goal box of the previous trial, sequential trials never required the same response. To ensure that participants attend to contexts, we added catch trials on 10% of the trials. A catch trial was indicated by either the context pattern (dots) or color (pink), and the task was not to respond to the task cue but to press the space bar with the thumb.

Procedure

The experiment was conducted online on the private devices of the participants. A minimum browser resolution of 1280 x 700 px was required to start the experiment. After providing informed consent, the participants received instructions and performed a training block. If participants failed to provide at least six correct responses in the first ten trials of the training block, instructions were presented again. If they failed this attention check a second time, the experiment was terminated. After finishing 43 training trials, participants worked on ten experimental blocks each containing 64 trials. Trial order was determined by an algorithm so that N-2→N-1 task transitions, N-1→N task transitions, N-2→N-1 context transitions, N-1→N context transitions were orthogonally balanced, i.e., each combination of these factors appeared equally often per block. Each participant was paid 4.50 £ after finishing the experiment.
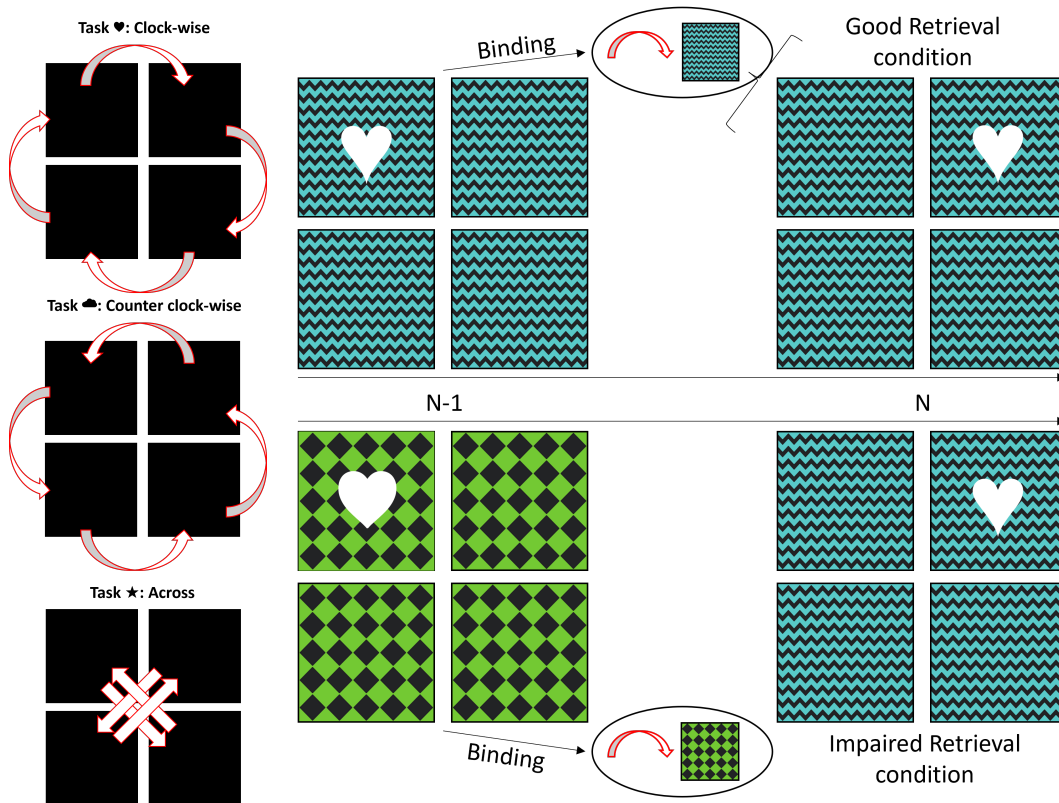
FIGURE 17: On the left side of the figure, the goal boxes for each of the three used tasks (clockwise, counterclockwise, and across) are visualized in dependency of the starting box. On the right side, an example context transition across sequential trials is shown visualizing the expected binding between the task rules and the visual context feature.

Results

Before analysis, we applied the preregistered trial outlier criteria and excluded all catch trials, trials following catch trials, the first trial of each block, trials involving backward inhibition task sequences (A→B→A tasks sequences, see e.g. Koch et al., 2010), and trials following error trials from analysis. For RT analysis, we also excluded error trials and trials deviating more than 3 SD from the individual factorial cell mean. In total we excluded 31.5 % of the trials from analysis.

A repeated measures ANOVA with the factors task transition [task repetition vs switch] and context transition [context repetition vs change] was conducted for RTs and error rates. The RT results are visualized in Figure 18.

RTs. We observed two main effects: A main effect of task transition, $F(1, 44) = 382.43, p < .001, \eta_p^2 = .897$, because RTs in task repeat trials were shorter ($M = 617$ ms) than RTs in task switch trials ($M = 737$ ms), and a main

effect of context transition, $F(1, 44) = 25.84, p < .001, \eta_p^2 = .370$, because RTs
in trials that repeated the context of the previous trial were shorter ($M = 671$
ms) than in trials with a different context ($M = 683$ ms). Most importantly, a
significant two-way interaction between the factors task and context transition
was observed, $F(1, 44) = 16.25, p < .001, \eta_p^2 = .270$, because task switch costs
were higher in trials that repeated the context of the previous trial ($\Delta = 131$
ms) than in trials with a different context ($\Delta = 109$ ms).

Errors.   We observed a main effect of task repetition, $F(1, 44) = 26.71, p <
.001, \eta_p^2 = .378$, because error rates in trials that repeated the task of the
previous trial were lower ($M = 4\%$) than in trials with a different task ($M =
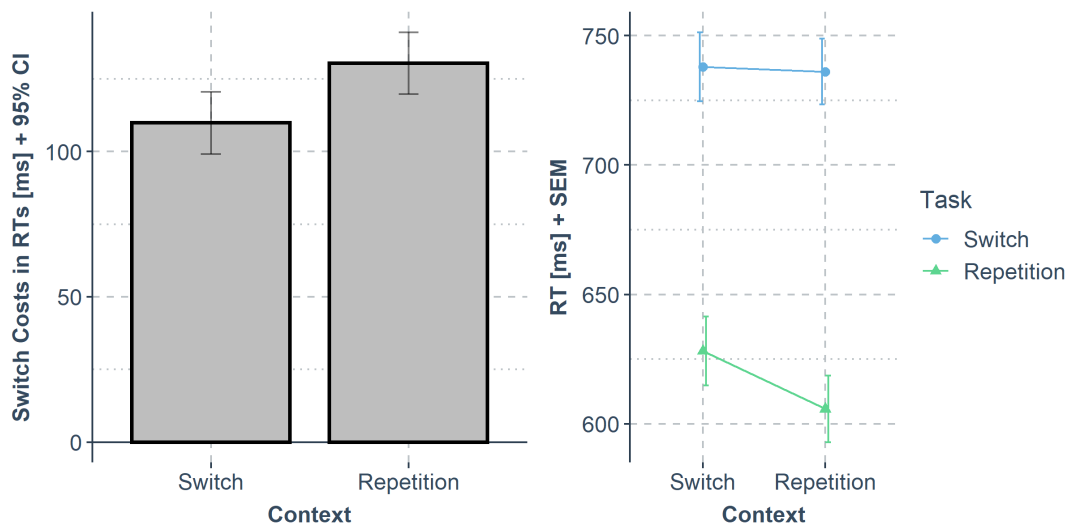7\%$). No other effect was significant ($p \geq .540$).

FIGURE 18: The left panel shows switch costs in RTs (y-axis; calculated as mean
RTtask switch – mean RTtask repetition; error bars indicate the 95% confidence
interval for the paired differences) dependent on the context transition (x-axis). The
right panel shows the same data in mean RTs (y-axis; error bars indicate the standard
error of the mean for each condition) dependent on the context transition (x-axis)
and task transition (color).

Experiment 2

An important difference between task switching paradigms using only two
tasks and those using three tasks (as in this study) is that the conditional
probabilities for the occurrence of not performed tasks in task switch trials
differ. If only two tasks are possible, a task switch necessarily means a switch
to the previously not performed task, whereas if three tasks are possible on a
task switch there is a 50 % chance for each of the previously not performed tasks

to occur. Thus, balancing task repetitions and switches (as in Experiment 1) means that the probability that the same task occurs as in the previous trial was 50 %, but the probability for each of the remaining tasks was only 25 %. This imbalance may have given participants an incentive to prepare the previously performed task, since out of the three it was the most likely task to occur. In Experiment 2, we aimed to replicate the findings of Experiment 1, but, instead of equally balancing the probability for task and context transitions, we balanced the trial order so that each of the three tasks and contexts could appear with the same probability. In this way, the chance for each task and context to occur was independent from the previous trial and therefore, there was no incentive to prepare the previous tasks or contexts.

Methods

The hypothesis, procedure, outlier criteria, methods, and planned analysis were preregistered on OSF (https://osf.io/ktxrm). Raw data, scripts for the experiments, and analysis are available on OSF.

Participants

We analyzed a sample of 104 participants (45 female, 55 male, 4 diverse; mean age: 27). A power analysis indicated a sample size of $N = 103$ to achieve a test power of 90% with a .5 alpha criterion to observe an effect size of dz 0 .291. This effect size was estimated based on N-2→N binding and retrieval effects (see Discussion and Appendix) observed in a pilot study. One participant was collected additionally due to a technical error. The recruitment criteria were identical to Experiment 1, but participants who took part in Experiment 1 were excluded. Two participants were excluded from analysis due to an error rate higher than 30 %. Both participants were replaced.

Task, stimuli, and procedure

The experiment was structurally identical to Experiment 1, but the probability for task and/or context repetition across trials was reduced to 33% (in Experiment 1: 50%).

Results

The same analysis plan as for Exp.1 was conducted for Exp. 2 (45.5% of the trials excluded from analysis due to the preregistered exclusion criteria). The RT results are visualized in Figure 7.

RTs. We observed two main effects: A main effect of task transition, $F1, 103) = 188.43, p < .001, \eta_p^2 = .647$, because RTs in task repeat trials were shorter ($M = 709$ ms) than in task switch trials ($M = 774$ ms), and a main effect of context transition, $F1, 103) = 10.03, p = .002, \eta_p^2 = .089$, because RTs in trials that repeated the context of the previous trial were shorter ($M = 738$ ms) than in trials with a different context ($M = 745$ ms). Finally, a significant two-way interaction was observed, $F1, 103) = 8.61, p < .001, \eta_p^2 = .077$, because task switch costs were larger in trials that repeated the context of the previous trial ($\Delta = 72$ ms) than in trials with a different context ($\Delta = 58$ ms).

Errors. The same analysis on error rates resulted in no significant effect ($p \geq .138$).
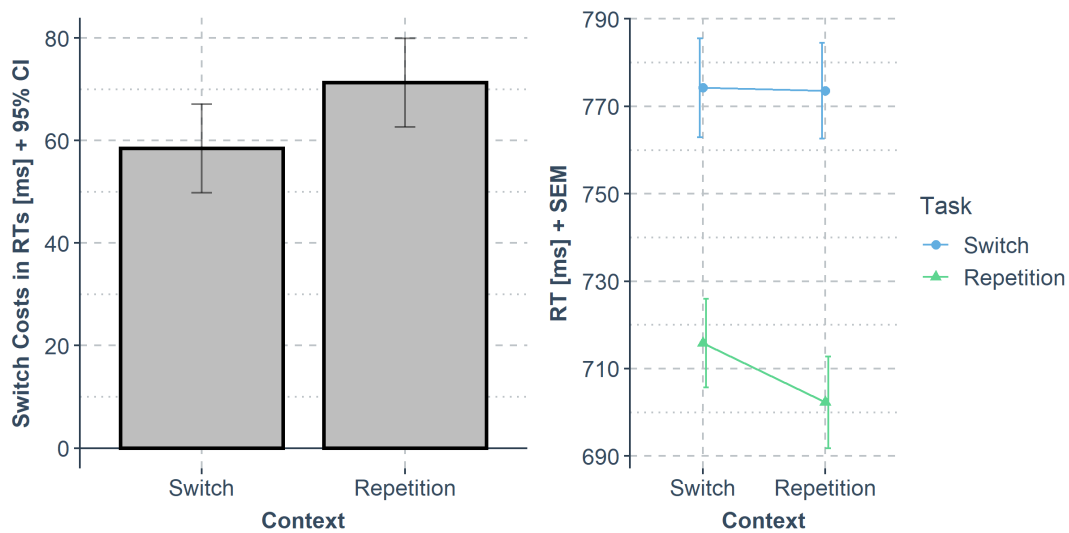

FIGURE 19: The left panel shows switch costs in RTs (y-axis; calculated as mean RTtask switch – mean RTtask repetition; error bars indicate the 95% confidence interval for the paired differences) dependent on the context transition (x-axis). The right panel shows the same data in mean RTs (y-axis; error bars indicate the standard error of the mean for each condition) dependent on the context transition (x-axis) and task transition (color).

Experiment 3

Experiments 1 and 2 used one task cue for each task, so task repetitions were also task cue repetitions. Empirical studies have shown that a performance benefit in cue repetition trials exists beyond switch costs (Forstmann et al., 2007; Logan & Bundesen, 2003; U. Mayr & Kliegl, 2003) and both processes are dissociable on a neurophysiological level (Jost et al., 2008). Further, it has been suggested that retrieving visual stimulus features independently from response

features can improve performance if the retrieved stimulus features match the currently perceived stimulus features and impair performance if there is a feature mismatch (Giesen & Rothermund, 2014). Regarding Experiments 1 and 2, both described mechanisms could provide an alternative explanation to bindings between abstract task rules and the context: Either participants may have been able to encode task cues faster if the context repeated, or stimulus-to-stimulus bindings between the task cue and the context supported the processing of the task cue. To address these alternative explanations, we conducted a third experiment in which two task cues were mapped to each task. Trial order was adjusted so that task cues never repeated across trials. Because the task cue always changed, binding effects cannot be the result of visual encoding benefits, or bindings between the task cue and the context.

Methods

The hypothesis, procedure, outlier criteria, methods, and planned analysis were preregistered on OSF (https://osf.io/nryw8). Raw data, scripts for the experiments, and analysis are available on OSF.

Participants

Following the same sample size reasoning as in experiment 2, we collected a sample of 103 participants (47 female, 52 male, 4 diverse; mean age: 28). The recruitment criteria were identical to the previous experiments. One participant was replaced due to an error rate higher than 30 %.

Participants

The experiment was structurally identical to the previous experiments. The main difference was that we used a 2:1 task cue to task mapping and adjusting trial order so that task cues never repeated across trials. Following the largest reported effect size for switch costs with 2:1 mappings in the work of Schneider and Logan (2011), we used semi-explicit task cues: "I" or "M" for *clockwise* (in German "IM Uhrzeigersinn"), "G" or "E" for *counter-clockwise* ("GEgen Uhzeigersinn"), and "K" or "R" for *across* ("KReuzweise"). The probability of context repetitions across trials was 33 %, while the probability for task repetitions across trials was 50 %.

Results

The analysis plan remained identical as in the previous experiments (37.1 %
of the trials excluded from analysis due to the preregistered exclusion criteria).
The RT results are visualized in Figure 9.

RTs. We observed two main effects: A main effect of task transition,
$F(1, 102) = 325.61, p < .001, \eta_p^2 = .761$, because RTs in task repeat trials were
shorter ($M = 756$ ms) than in task switch trials ($M = 878$ ms), and a main
effect of context transition, $F(1, 102) = 19.87, p < .001, \eta_p^2 = .163$, because RTs
in trials that repeated the context of the previous trial were shorter ($M = 810$
ms) than in trials with a different context ($M = 823$ ms). Finally, a signifi-
cant two-way interaction was observed, $F(1, 102) = 4.02, p = .048, \eta_p^2 = .038$,
because task switch costs were larger in trials that repeated the context of the
previous trial ($\Delta = 129$ ms) than in trials with a different context ($\Delta = 115$
ms).

Errors. The same analysis on error rates resulted in a significant main effect
of task repetition, $F(1, 102) = 22.37, p < .001, \eta_p^2 = .180$, because error rates in
task repeat trials were lower ($M = 4.4\%$) than in task switch trials ($M = 6.2\%$).
No main effect of context transition was observed ($p > .74$). Descriptively, the
two-way interaction in error rates showed the same data pattern as in RTs, but
the effect was not statistically significant, $F(1, 102) = 3.07, p = .083, \eta_p^2 = .029$.

Introduction

The current study tested whether task rules that guide the translation of
stimulus input into motor output can be bound and retrieved. Importantly,
these task rules act independently from specific response codes, i. e. benefits
for task repetitions are observable even without response repetitions. In three
experiments, we used a task switching paradigm with three spatial operation
tasks and combined it with a task-irrelevant visual context feature. According
to binding theories adopted task rules and context features should be bound
and repeating the context feature in the next trial should facilitate the retrieval
of these task rules (Frings et al., 2020). Consequently, on context repetitions,
performance on task repetitions should be improved compared to task switches,
i.e., the costs to switch tasks should be larger. In line with these predictions,
we found that in all three experiments there was an increase in switch costs
on context repetition trials. Critically, due to the design, tasks could not be
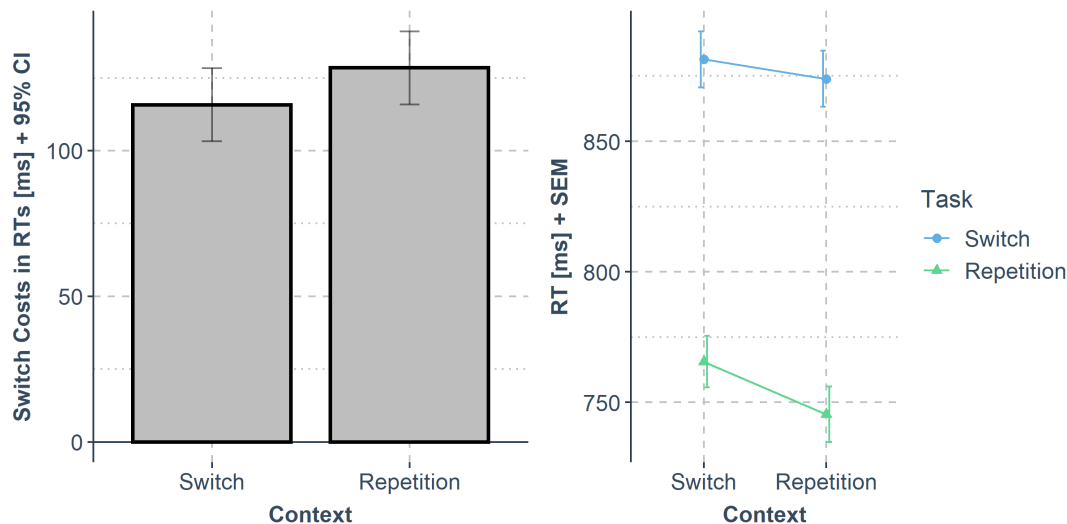
FIGURE 20: The left panel shows switch costs in RTs (y-axis; calculated as mean RTtask switch – mean RTtask repetition; error bars indicate the 95% confidence interval for the paired differences) dependent on the context transition (x-axis). The right panel shows the same data in mean RTs (y-axis; error bars indicate the standard error of the mean for each condition) dependent on the context transition (x-axis) and task transition (color).

distinguished by their response options. Going beyond previous research (e. g., Koch, Frings, & Schuch, 2018; Schuch & Keppler, 2022), the observed increase of switch costs in context repetition trials therefore cannot be attributed to response retrieval. Instead, we suggest that context repetitions facilitated the retrieval of response-independent task rules.

In Experiments 1 and 2, a single task cue was mapped to each task. Consequently, task repetitions were also task cue repetitions. Encoding benefits that result from the context being repeated together with the task cue (Jost et al., 2013), or bindings between the context and the task cue (Giesen & Rothermund, 2014) provide alternative explanations to the observed binding effect. To address these alternative accounts, we mapped two task cues to each task and avoided task cue repetitions by design in Experiment 3. We successfully replicated the binding effect of the first two experiments, which strengthens the conjecture that response-independent task rules can become bound to context features. Although descriptively the observed binding effect was smaller in Experiment 3 ($\Delta = 14$ ms) than in the previous experiments (Exp. 1 $\Delta = 20$ ms; Exp. 2 $\Delta = 23$ ms), no significant difference in the binding effect between the experiments was observed when combining all data in a single ANOVA with experiment as a between-subjects factor.

TABLE 10: Mean response times (RT) and error rates for each trial condition in Experiments 1, 2, and 3 and the resulting switch costs.

	Experiment 1		Experiment 2		Experiment 3	
	RT	**Errors**	**RT**	**Errors**	**RT**	**Errors**
Context repetition						
Task repetition	606 [± 12.8]	3.6 [± 0.5]	702 [± 10.5]	4.6 [± 0.4]	745 [± 10.6]	4.2 [± 0.4]
Task change	736 [± 12.8]	7.4 [± 0.9]	773 [± 11.0]	4.7 [± 0.3]	874 [± 10.7]	6.4 [± 0.5]
Switch Costs	130 [± 6.7]	3.7 [± 0.7]	71 [± 5.4]	0.1 [± 0.4]	129 [± 8.5]	2.2 [± 0.5]
Context Change						
Task repetition	628 [± 13.3]	3.5 [±0.4]	716 [± 10.1]	4.2 [± 0.2]	766 [± 9.9]	4.5 [± 0.3]
Task change	737 [± 13.3]	7.2 [±0.9]	774 [± 11.3]	4.9 [± 0.3]	881 [± 10.7]	5.9 [± 0.4]
Switch Costs	110 [± 6.6]	3.7 [± 7.6]	58 [± 5.0]	0.7 [± 0.3]	115 [± 6.3]	1.4 [± 0.4]

Note. Switch costs were calculated as $RT/\text{error}_{\text{task change}} - RT/\text{error}_{\text{task repetition}}$. Standard errors in brackets.

Showing that task rules can be bound and retrieved generalizes the notion of binding mechanisms to more abstract, non-perceivable aspects of task sets going beyond previous research investigating effects of stimulus and response bindings in task switching (e. g., Koch, Frings, & Schuch, 2018; Schuch & Keppler, 2022). Further, this provides an ecologically more valid perspective on the interplay of task switching and binding processes: Repeating the same task only rarely entails an exact repetition of the previous action. Rather, novel actions to novel stimuli must be performed in service of reaching an unchanged (i.e., repeated) task goal as the appropriate action must be selected under consideration of other environmental information (after performing the task "picking flowers", a task repetition necessarily requires a translation of the environmental information into new actions, since exact action repetitions will only lead you to the exact same spot where no more flowers are left).

Further, the observed effects of bindings between task rules and context features are in line with findings of studies using similar approaches to investigate context effects on other abstract cognitive states such as attentional weights. E. g. in response conflict paradigms (such as the Flanker task), the *Congruency Sequence Effect*, a behavioral effect supposedly reflecting control adaptation (Egner, 2017), is significantly larger if task irrelevant context features repeat across trials than if they change. This effect is attributed to bindings between

context features and cognitive parameters controlling the attentional weights that are allocated towards distractor and target information (Dignath et al., 2019; Dignath & Kiesel, 2021; Dignath et al., 2021; Grant et al., 2021; Jiang et al., 2015; Spapé & Hommel, 2008).

It remains an open question to what extent bindings such as those operationalized in this study relate to other forms of contextualized control, in which typically context is instructive of task demands and these contingencies can be learned over time (Bugg et al., 2011; Crump et al., 2006; Crump & Logan, 2010; for reviews see Bugg, 2017; Bugg & Crump, 2012). Oberauer et al. (2013) have developed a computational models of task control with two learning systems: one for fast-changing bindings between task features that hold active in working memory, and one for slow-changing associations in long-term memory. Here the more recent experiences can have strong influence on behavior via bindings, but they transfer slowly to long-term memory. Following a similar idea, Giesen et al. (2019) found that the episodic retrieval of stimulus-response bindings provide access to the most recent occurrence of the current situation. Such approaches could be employed to test to which extend bindings can account for contingency learning between contexts and task demands (see above) or one-shot learning of context to control associations (Brosowsky & Crump, 2018; Whitehead et al., 2020).

Two limitations of the present study should be noted. First, in all three studies, task switch sequences appeared unaffected by context transitions, i. e. performance was not impaired on context repetitions compared to context changes, as predicted by binding theories (Frings et al., 2020). Two explanations for this pattern seem plausible. One possibility is that we observed an effect of context transitions beyond the presumed effect of context to task rule bindings. While task rule binding predicts that task switches should be more difficult on context repetitions than on context changes, it's conceivable that the repetition of the context facilitates performance. One possible explanation for the observed benefits of context repetition is that it might provide improved encoding conditions. Alternatively, participants could spend time scrutinizing the constellation of context features to detect catch trials, and this process is likely to be faster if the context repeats than if it changes. Such an overlap of two effects could descriptively offset binding effects in task switch sequences but amplify them in task repetition sequences. On the other hand, it's also possible that we observed an interaction effect between the factors of task transition

and context transition, and this interaction was primarily driven by the interplay of task repetitions and context transitions. This perspective challenges the assumption that, on task switch trials with context repetitions, the task rules from the previous trial are retrieved. Alternative explanation could be that a changing context may disrupt actively maintained task rules (for such a perspective in conflict adaptation see Kreutzfeldt et al., 2016). With the current dataset, we cannot definitively exclude either possibility, but studies utilizing electrophysiological measures to investigate task rule bindings have begun to explore the retrieval process (Rangel et al., 2023). Possibly, similar approaches could help to determine whether the effects of context to task rule bindings should be described as a result of retrieval processes or alternative mechanisms, such as disruption.

A second limitation could be that previous research indicates that the processing of contextual novelty shares neural networks with task updating processes (Barcelo et al., 2006) and error processing (Wessel et al., 2012). Assuming that participants are biased to expect more context repetitions, contextual changes might elicit surprise and thereby impair performance which is potentially mostly reflected in the faster task repetition trials. However, this explanation appears unlikely because binding effects were also observed in Experiment 2 in which context repetitions were less likely than context changes. Thus, one would have to make the additional assumption that the repetition bias in expectations was independent from the actual proportion of context transitions.

To sum up, in three experiments, we observed increased switch costs in trial sequences repeating a visual context feature compared to context changes. Since the design of the paradigm controls for response retrieval, we suggest that the observed effects result from bindings between the visual context and response-independent task rules that guide the translation of stimulus input into response output. These findings add to the growing body of literature on the interplay of task switching and binding processes by demonstrating that abstract parts of task sets can be bound and retrieved independently of specific response codes.

Declarations

Ethical approval: The present study fulfils the requirements of the generalized ethical approval by the Ethics Committee for Psychological Research, University of Tübingen, Germany.

Author contributions All authors contributed to the study concept and design. Moritz Schiltenwolf performed programming, testing, data collection and analysis. Moritz Schiltenwolf drafted the article, and David Dignath and Eliot Hazeltine provided critical revisions. All authors approved the final version of the article for submission.

Open science statement: The data, materials, analysis scripts and preregistrations for both experiments are available at https://osf.io/z9twy/.

Chapter 4

Results and outlooks

4.1   Summary of results

The time course of abstract control states and control bindings

The first objective of this thesis revolves around examining the temporal dynamics of control states. Different aspects of this subject were explored, including the influences of the abstraction level of adopted control states and their contextual bindings.

The first explored question asked whether abstract control states that act independently from specific stimulus and response codes would decay over time. Such results would replicate previous findings that reported the decay of control states possibly operating on less abstract levels (Duthoo et al., 2014a; Egner et al., 2010). All four experiments of Study I and, although not specifically designed for this hypothesis test, four out of five experiments of Study II provided evidence in favor of invariant CSE sizes across increasing time intervals between the control-inducing and control-probing trials. This can be interpreted as robust evidence for the temporal stability of abstract control states adopted in the applied paradigms. Certainly, for the time being, this stability assumption only holds for time intervals within a range of seconds, with a maximum of nine seconds, as implemented in Experiment 4 of Study I.

Second, Study I provided in two experiments a direct comparison of the time course between abstract control states, as induced in confound-minimized designs, and those that can include nonabstract bindings between specific stimulus and response features since these features can repeat across trials. Interestingly, the results were mixed. Experiment 2 reported temporally stable CSEs in both conditions, with no significant differences in the respective CSE slopes. However, Experiment 3 indicated for the *feature repetition* condition a clear reduction in the CSE across time in RTs and error rates, but not for the CSE in the *confound-minimized* condition. A comparison of the respective slopes indicated a significant difference between the two conditions.

Finally, Study II addressed the time course of bindings between visual contexts and abstract control states. Here, the measure of interest was the C-CSE, as previous studies suggest that it reflects the size of control binding effects (Dignath et al., 2019; Dignath & Kiesel, 2021; Spapé & Hommel, 2008). In five

individual experiments, we did not observe decisive evidence indicating that the C-CSE is sensitive to the duration of the interval between the prime trial during which the adopted control state becomes bound to the context and the probe trial where effects of control retrieval are measured (short intervals lasting for 500 ms, long intervals ranging from 2000 to 5000 ms). Utilizing the high structural similarity between the experiments, a mega-analysis was conducted by pooling the data of all the experiments in a single analysis while controlling for between-experiment influences. This integrative approach provided decisive evidence against an influence of the interval duration on the C-CSE. This result indicates that context-to-control bindings follow a different, longer persisting time course than bindings between perceivable features such as stimuli and responses (e.g., Hommel & Frings, 2020). However, these results should be interpreted with some caution, as we observed surprisingly large variance in the effect sizes of control bindings, indicating limited measurement accuracy for the effect of interest.

The C-CSE explained by control retrieval versus disrupted control

Study III compared two different mechanisms that were proposed as explanatory approaches for the C-CSE. The retrieval account posits that context can serve as a cue to retrieve the control state bound to it (Frings et al., 2020). In contrast, the attentional reset account suggests that changes in context disrupt the maintenance of control states. To discern these accounts, we employed a Bayesian statistical approach to compare evidence in favor of a modulating influence of the factor N-2→N context transitions (ABA vs CBA context sequences) on the N-2→N control adaptation effect against a model without such an interaction term. In three experiments, we observed decisive evidence against such a modulation of N-2→N control adaptations by N-2→N context transitions, indicating that C-CSE are aptly explained by attentional reset rather than control retrieval. Furthermore, in Experiment 1, we observed an unpredicted, but surprisingly strong modulating influence of response conflict on the interplay between context transitions and control adaptations, indicating that experiencing response conflict can have a substantial influence on the maintenance of context-to-control bindings. In Experiment 3 of this study, we controlled for the factor response hand transitions by adjusting the trial order so that participants would always switch their response hand from trial to trial. In this study, we did not observe an N-2→N control adaptation effect, indicating that

the response effector possibly acts as an additional context, inducing greater disruption of control state maintenance through response hand changes across consecutive trials.

A generalization of abstract control bindings to task control

Study IV of this thesis aimed to test whether abstract task rules controlling task-appropriate behavior independently of specific stimulus or response codes can be bound to visual contexts. In three experiments, SCs increased significantly if a visual context that was presented during task performance repeated across trials in comparison to context changes. These findings suggest that the abstract task rules of the previous trial become bound to the visual context. Consequently, when the context repeats, it becomes easier to repeat the previous task, whereas switching to a new task becomes more challenging. Importantly, these findings held true even after controlling for potential encoding benefits resulting from sequential task cue repetitions (Giesen & Rothermund, 2014; Logan & Bundesen, 2003). The findings of this study provide the first evidence that abstract task rules can indeed be bound to observable features, thereby providing an important synthesis between findings of bound attentional control states (e.g., Dignath et al., 2019; Dignath & Kiesel, 2021) and binding research in task switching, where most studies have focused on the influence of stimulus-to-response bindings (e.g., Benini et al., 2022a; Schuch & Keppler, 2022). Taken together, the findings of this study extend the concept of bindings in task control to encompass abstract control states, illustrating the versatility and adaptability of such binding mechanisms.

4.2 Implications and open questions

In this concluding section of my thesis, I will explore the implications of my findings and highlight the research questions that could be derived from them. I structure this discussion into three parts. First, I examine the integration of the temporal factor into cognitive control theories. Second, I explore a potential framework for control bindings that does not require a retrieval process, as outlined in episodic binding and retrieval theories. Third, I discuss how control bindings on different abstraction levels can be integrated into a unifying framework of task control.

Time and cognitive control

A motivation behind this thesis was to conduct empirical investigations of the time course of control states to develop an approach for how to incorporate the factor time into models of cognitive control. As outlined in the introduction, influential models either implement time as discrete trial steps (Botvinick et al., 2001; Jiang et al., 2014) or speculate about proactive functions of control that imply an extended durability without specifying the speculated temporal scales (Braver, 2012; Gratton et al., 1992).

Empirical studies have reported that the CSE decreases with increasing delays between control-inducing and control-probing trials, suggesting a temporal decay of control states (Duthoo et al., 2014a; Egner et al., 2010). However, these findings are constrained by the use of task designs with sequential feature overlap, possibly indicating that the authors did not measure the decay of abstract attentional weights but rather the decay of bindings between stimulus and response features, providing control on nonabstract levels (Hommel et al., 2004). This reasoning aligns with the literature on the time course of stimulus-to-response bindings (in the absence of response conflict), which report that these types of bindings decay within a similar time as, for instance, reported by Egner et al. (2010; see, e.g., Hommel & Frings, 2020).

In Study I of this thesis, we explored whether control states that act on abstract levels, that is, independently from specific stimulus or response features, would be temporally more stable than potentially less abstract control states that could act on stimulus-to-response bindings, similar to prior studies exploring the time course of the CSE (Duthoo et al., 2014a; Egner et al., 2010). Across

several studies, abstract control states were found to be resilient against tempo-
ral delays, whereas comparisons with nonabstract control states were mixed, as
they seemed to decay in one experiment but not in another. The interpretation
of the time course of nonabstract control states becomes even more complicated
in consideration that also Duthoo et al. (2014a) observed them to be durable
for several seconds if the time interval was predictable.

Building on Badre's (2008) proposition that abstract representations main-
tain their relevance over an extended period, one might speculate that control
states operating on less abstract levels, specifically focusing on individual stimuli
and response codes, are pertinent only to the immediate situation, as these con-
trol states cannot facilitate conflict resolution when stimuli or responses change.
Conversely, abstract control states, operating independently from specific ob-
servable feature codes, should be regarded as a generalization of abstract fea-
ture relations encountered across various instances. Consequently, an abstract
control state provides benefits only when it is anticipated that an applicable
situation will be encountered; hence its extended temporal relevance. While
the empirical studies on this topic generally support such reasoning (studies I
and II; Duthoo et al., 2014a; Egner et al., 2010), it remains unclear how the
temporal durability of control states is derived from its abstraction level.

A straightforward answer to this question might be to assume that in studies
with stimulus and response repetitions across trials that reported a temporally
decreasing CSE (Egner et al., 2010; Exp. 1 in Duthoo et al., 2014a; Exp. 3
in Study I), decaying stimulus-to-response bindings were observed. In contrast,
abstract control and abstract control bindings may reflect different mechanisms
or bindings of features with different time courses. However, taking into ac-
count that the literature on the topic of context-specific control states shows
that control processes at all abstraction levels involve closely interconnected
learning mechanisms, the assumption of a mechanistic dichotomy between ab-
stract and nonabstract control seems questionable (Egner, 2014; Bugg et al.,
2008; Bugg et al., 2020; Dignath et al., 2019).

Nevertheless, the type of features upon which control acts is an interest-
ing candidate for a factor that could determine its time course. The literature
examining the time course of bindings suggests that all stimulus-to-response
bindings decay over time (Frings, 2011; Frings et al., 2022; Hommel & Frings,
2020). Notably, bindings between two responses in sequential trials appear to

be temporally stable (Moeller & Frings, 2021), suggesting that it might not be the bound representations of response features that decay. Instead, this potentially implies that the bound representations of stimulus features, which serve as retrieval cues, decay over time. This notion is challenged by Study II of this thesis, which reports that control states bound to a visual context remain temporally stable. However, one might argue that, in this study, the contextual format itself was already abstract to a certain degree, as it was not the stimulus identity that acted as a context but rather the category of the number stimuli, for instance, number words. Future experiments could explore whether bindings using categorical stimulus features as retrieval cues have a different decay function than those using the identity of stimuli as the retrieval cue.

An alternative perspective on the question of the temporal stability of control states could focus on the question of why control should or should not be temporally stable (Braver, 2012). Computational models usually describe control adaptations as the continuous learning of task demands (Botvinick et al., 2001; Jiang et al., 2014; Verguts & Notebaert, 2008). It is conceivable that such a learning system could also integrate information about the expected relevance of adopted control states. To employ abstract control, the system must identify abstract, typically relational features that persist across multiple trials. Given that such a learning process relies on information from past trials, it is plausible that successful learning necessitates sustained maintenance as an integral part of the control state (Schultz et al., 1997). However, if the system does not select abstract features as the leverage point of cognitive control, such a learning process across trials becomes unnecessary; thus, control maintenance might not be selected as a part of the control state itself. Interestingly, Jiang et al. (2014) introduced the concept of expected relevance of a control state by modeling control adaptations as a Bayesian learning mechanism with varying learning rates. In situations where task demands become more volatile, indicating that the presently implemented control state is usually pertinent for only a limited number of events, the model's learning rate increases, thereby discounting previous experiences. Conversely, if control states remain applicable across various events, the system preserves them for an extended period by reducing the influence of upcoming events. Although in this model, time is reflected as passing trials and relevance rather reflects the shielding of a control state against new influences, such Bayesian approaches could be intriguing for implementing a model that describes the need for control maintenance.

From such a learning perspective, the abstraction level of control states does not directly determine its time course. Instead, the relevance of the information of the current trial is to support the learning process in the upcoming trial. This approach could potentially be experimentally tested by manipulating the relevance of task features across trials in situations in which participants usually rely on nonabstract, fast-decaying control states. For instance, relevance could be increased by adding a secondary task in which participants have to detect contingencies of task features across trials. If learning processes across trials indeed increase the need for maintenance, the effects of control adaptations should have prolonged time courses under such conditions.

Control bindings without control retrieval

Study II of this thesis aimed to test whether a retrieval process, as described by the episodic binding and retrieval accounts (Beste et al., 2023; Frings et al., 2023; Frings et al., 2020; Hommel, 2009), could account for control binding effects, such as those reflected by the C-CSE. Given the varying definitions of the term "retrieval" in memory research, I will briefly recapitulate how the retrieval process was defined in episodic binding and retrieval accounts. First, when feature representations simultaneously attain sufficient activation, they bind. Upon the reactivation of any feature representations of such a binding, all other bound features are retrieved, leading to an increase in their activation. Importantly, if a binding is not retrieved it should remain unchanged. Consequently, an existing binding should be unaffected by subsequent events in which no feature overlaps with the binding. However, Study III of this thesis did not reveal any effects of control bindings after they were followed by a trial without feature overlap with the binding trial. Consequently, these results challenge the described retrieval process, prompting the question of how effects of control binding can be explained without relying on a retrieval process as proposed by episodic binding and retrieval accounts.

Kreutzfeldt et al. (2016) proposed an alternative perspective, suggesting that control states are proactively maintained and that context changes trigger an attentional reset, disrupting control maintenance and thereby reducing trial-by-trial control adaptation effects when context changes occur between control-inducing and control-probing trials. Although the attentional reset account appears, at first glance, to be parsimonious, it necessitates additional assumptions.

One challenge stemming from empirical findings is to determine which types of feature changes disrupt control maintenance and which do not. For example, why does the sequential alternation of stimulus and response features in confound-minimized task designs not lead to control disruptions (Gyurkovics et al., 2020; Jiménez & Méndez, 2013; S. Kim & Cho, 2014; Weissman et al., 2014)? Even changes within the dimension of context features uncorrelated to task demands, as used in the standard paradigm to measure the C-CSE (Dignath et al., 2019), seem to elicit attentional reset only under certain conditions. Grant et al. (2020; Grant et al., 2021) reported that a C-CSE was observed only when each trial could be distinctly assigned to a specific context. However, if such an assignment was not possible, because the context of the target and the distractor dimensions could vary independently from each other, the CSE remained invariant to contextual changes. Based on these findings, it seems that an attentional reset account may be insufficiently specified if it solely considers trial-by-trial context changes. Instead, it appears that an additional learning mechanism is required to detect the features that serve as boundaries for the currently implemented control state (Grant et al., 2020).

An interesting perspective on this issue comes from gating accounts in the working memory literature (see, e.g., Chiew & Braver, 2017; Ott & Nieder, 2019). These accounts propose a close relationship between the selection, maintenance, and updating of task-relevant features [1] and cognitive control functions (for a review on this topic, see also Meier & Kane, 2017). To be more specific, these accounts assume that a central requirement to accomplish goal-directed behavior is the maintenance of features that not only represent a declarative action goal but also support attention, action planning and execution. Furthermore, a mechanism is specified that selects features as task-relevant and controls when to maintain or shield task features ("closed gate") and when to update them ("open gate"). Interestingly, it was suggested that both mechanisms are controlled by a reinforcement learning system. First, this system allows to learn that certain occurring features are predictive of reward (Braver & Cohen, 2000). Subsequently, these learned reward predictions can be used to control whether currently active features need to be updated or maintained (Braver et

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[1] Here, it should be noted that gating accounts often use the term context for task-relevant features. However, both the definition and the approach differ from the context definition as used in this thesis, where context is defined as task features that are objectively (i.e., from the experimenter's point of view) decorrelated from task demands. On the contrary, gating accounts define context as all task features that are helpful for task performance from a subjective point of view (i.e., from the experiment participant's perspective; e.g., Chiew & Braver, 2017).

al., 1999). While the computational models derived from these accounts usually predict behavior in comparably simpler tasks, in which participants must maintain or update task rules depending on a probabilistic cue, their base principles might be helpful to achieve a more extensive account of attentional reset. Reward-based learning could be the mechanism that detects task features later acting as context. Conflict experiences are discussed as aversive (for reviews, see Dignath et al., 2020; Dreisbach & Fischer, 2012); consequently, reward would be expected for trial sequences with repeated congruency conditions so that maintained control states minimize conflict experiences. Features that are presented with such sequences might become predictive of reward and can subsequently act as context, whereas such learning processes should be impaired for features that never or rarely repeat across trials. Such features could then control the gating process by shielding maintained representations that support, for example, attentional control functions or whether active representations must be updated, thus discounting previously instated representations.

Hierarchies and abstraction levels of bindings in task control

Previous studies have explored how nonabstract task features, such as responses, impact behavior in task switching (e.g., Benini et al., 2022a, 2022b; Kandalowski et al., 2020). These studies indicate that phenomena such as the task-specific response repetition benefit can be attributed to the binding of response features with other features of the task set. However, these effects appear to be limited in scope, rendering it unclear to what extent such binding effects can be extrapolated beyond controlled laboratory conditions, where exact motor pattern repetitions are frequently encountered, to ecologically more valid situations, where task demands and required responses align on a categorical level but not within their specific identities. The aim of Study IV was to address this gap. As presented in the previous sections, the findings suggest that bindings of abstract features, such as response-independent task rules, can also influence task switching behavior, suggesting a generalized binding mechanism. Nevertheless, it remains unclear how bindings of different abstraction levels are integrated into a unified model of task control.

Several theoretical and empirical studies should be considered for such an integrative model. Most task control theories assume a hierarchical structure

where abstract goal representations govern the selection of less abstract features, such as specific stimuli and responses (Hirsch et al., 2018; Koechlin et al., 2003; Korb et al., 2017; Lien & Ruthruff, 2004; Schneider & Logan, 2006; Schumacher & Hazeltine, 2016). According to these theories, simple bindings between stimuli and responses, such as, for instance, the costs of partial repetitions of stimulus and response features (Frings et al., 2007; Hommel, 1998; Rothermund et al., 2005), could be described as effects on lower hierarchy levels. These effects are modulated by higher hierarchy factors, such as the to be applied task rule. For example, the benefits of sequential response repetitions are evident in sequences with task repetitions but not during task switches (e.g., Kleinsorge & Heuer, 1999; Koch et al., 2011; Rogers & Monsell, 1995; Schuch & Koch, 2004; see also Kiesel et al., 2006). This task specificity of feature repetitions is highlighted by findings showing that even in situations in which two tasks require an identical response, response repetition benefits can be observed only if the priming response corresponds to the same task as the probe response (U. Mayr & Bryck, 2005). Importantly, such findings challenge task control models that suggest that task rules control feature selection by automatically increasing the activation level for task-relevant stimulus and response features. Instead, it indicates a mechanism that discounts feature activation originating from different, previously implemented task rules.

Episodic binding and retrieval accounts propose such a discounting mechanism, suggesting that under impairing retrieval conditions, such as partial repetitions of the previous episode, costs arise due to code confusion or unbinding processes (Mocke et al., 2023). Support for this perspective can be found in studies that investigate the interplay of task rules, context features, and required responses. In these studies, the typically observed costs of partial feature repetitions appear to be task specific. (Benini et al., 2022a, 2022b; Kandalowski et al., 2020; Koch, Frings, & Schuch, 2018). However, it seems questionable whether episodic binding and retrieval can provide an extensive perspective on task switching behavior since in these theories, no hierarchical structure is included (for a discussion, see Hommel, 2022). Consequently, it is presumed that all features, including the task rules, are equivalent in hierarchy to all other bound features. However, such a perspective cannot explain why task rule repetition benefits are less sensitive to response feature changes than vice versa (e.g., Benini et al., 2022a). In other words, a simple episodic binding and retrieval account falls short in explaining why the feedforward flow in these hierarchies seems to be stronger than the bidirectional influences.

Episodic binding and retrieval accounts have introduced the concept of intentional feature weighting, which describes how intentions can influence the activation levels of task-relevant features (Memelink & Hommel, 2013). This concept closely aligns with the concepts of higher-level task control, and prior studies in attentional control research have discussed whether such control parameters can become bound to context (e.g., Dignath et al., 2019; Spapé & Hommel, 2008). Study IV of this thesis provides explicit evidence for the effects of bindings between abstract task rules and visual contexts. However, this further complicates the question of how bindings on different abstraction levels can be integrated into a unified model since these findings suggest that lower-level visual context features can exert influence on the implementation of higher-level task rules.

Additional insights into the structure of task control come from a study of Kikumoto and Mayr (2020), who employed temporally resolved representational similarity analyses to explore the time course of electrophysiological signals during task switching that can be assigned to the representations of task rules, stimuli, responses and their bindings. Interestingly, their results indicate a cascade in the activation of the different representations, with early activation of task rule representations, followed by stimulus representations, before binding activation could be decoded. These results could be interpreted as early evidence that task rules must first be implemented before binding can take place, suggesting a hierarchy that can also be observed on a temporal scale.

An intriguing starting point for a task model that incorporates a hierarchical structure of task control and that can effectively simulate binding effects can be found in the computational work of Oberauer and colleagues (2013). This connectionist model outlines task control from a working memory perspective as the interplay of two modules (additional functions, such as long-term learning, are omitted here for brevity). First, a response selection module is responsible for modulating the connections between stimulus inputs and response outputs through a binding matrix, reflecting the current strengths of bindings between specific stimuli and responses. Second, a task selection module describes the task cue-driven retrieval of the currently required task set and its subsequent implementation. With the subsequently described minor adjustments, this model has the potential to simulate all previously mentioned binding effects in task switching.
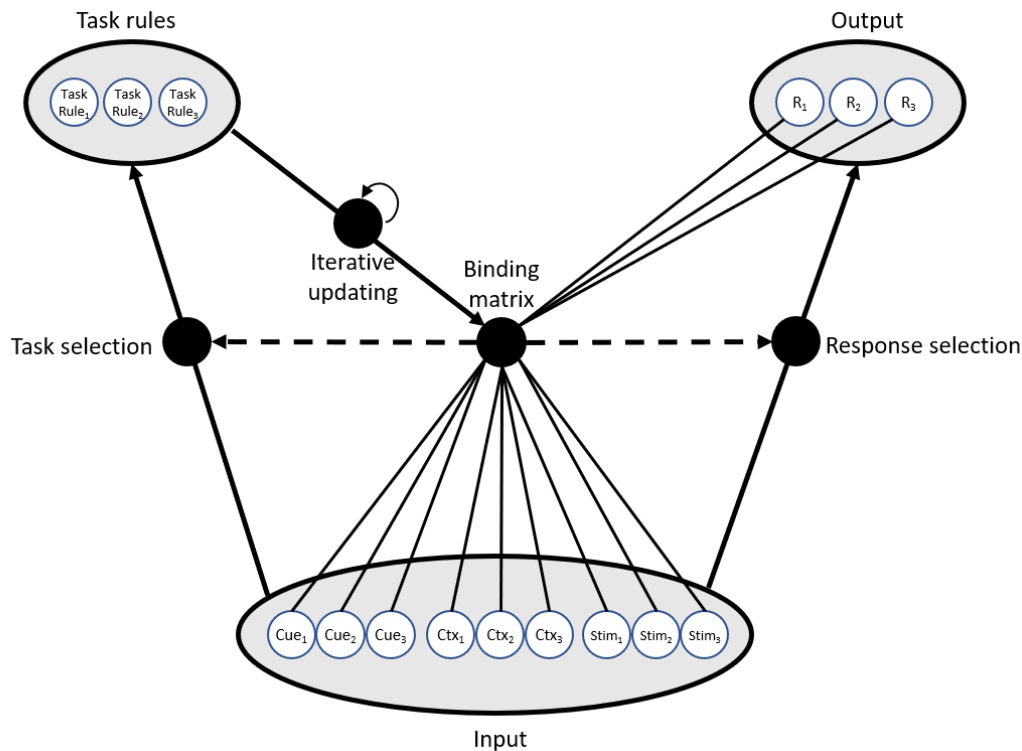
FIGURE 21: This figure illustrates the relevant modifications to the task switch model by Oberauer et al. (2013) that could enable the simulation of various binding and task switching effects. Central to the model is a binding matrix holding the connection weights between all task features, such as stimuli, task cues, contexts, and response features. This binding matrix can modulate the task selection process, allowing to account for abstract task rule bindings, as observed in Study IV, as well as the task selection process, enabling the model to account for traditional binding effects such as the costs of partial feature repetitions (e.g., Kunde & Wühr, 2006; Mocke et al., 2023). A hierarchical structure in task control, as proposed by most theoretical work (e.g., Schumacher & Hazeltine, 2016), is achieved through an iterative updating process that strengthens bindings described by the selected task rules and discounts all other bindings.

The most crucial extension involves modifying the binding matrix, which in the original work reflects stimulus input and response output on completely distinct dimensions. Adhering to the common coding principle in episodic binding and retrieval accounts (Hommel et al., 2001), the binding matrix should encompass all modeled task features and their respective binding strengths. This could be formalized as an autocorrelated vector containing all task features. Binding effects can be conceptualized as competing, mutually inhibiting accumulation processes of response activation (e.g., Usher & McClelland, 2001), where response activations are primed through the stimulus input and modulated by the

strength of bindings between features. In line with working memory assumptions of limited binding capacity (Oberauer, 2019), recent bindings should lead to reductions in the strength of older, nonidentical bindings, with a more pronounced impact on bindings that partially overlap in task-relevant features (see also Oberauer & Vockenberg, 2009). This implementation of bindings should be capable of simulating basic binding effects, such as the costs associated with partial stimulus repetitions in task repetition trials.

To account for the effects of task switches, Oberauer et al. (2013) describe a two-step task rule updating process. First, triggered by the task cue, a competitive task rule retrieval process begins, analogous to the response selection process. The retrieved task rule is represented in the model as a matrix containing the task-appropriate connection weights between the stimulus input, task-relevant features and response output. In a second step, the binding matrix is updated in an iterative process by the task rule matrix. With each iteration, the bindings specified in the task rule matrix are reinforced, while other bindings are discounted. The iteration process ends when the difference between the matrices falls under a critical threshold. This updating mechanism can account for the task-specific nature of stimulus-to-response bindings (e.g., Benini et al., 2022a) since during task switches multiple iterations of the updating process erase bindings not described by the task rule matrix, whereas task repetitions require fewer iterations.

Finally, effects of task rule bindings, as observed in Study IV, could be modeled as influences of bindings on the task rule retrieval process. If the currently present context is bound to the present task cue, the retrieval of the correct task rule is facilitated, as the strength of the binding should speed up the accumulation process for the retrieval of the correct task rule. However, if the context is bound to a different task cue, a competing retrieval process for an inappropriate task rule is amplified, increasing the time required for the retrieval of the correct task rule and raising the risk of retrieving an incorrect task rule. Effects of context-to-task rule bindings, independent of specific task cues, as observed in Experiment 3 of Study IV, could be explained as a task rule retrieval process that is mediated by a semantic description of each task rule (e.g., participants might memorize task cues "I" and "M" as the semantic description "clockwise"). Bindings that incorporate these semantic features (Henson et al., 2014; Horner & Henson, 2011; Laub & Frings, 2020) could then influence this process.

Extending the working memory model for task switching by Oberauer et al. (2013) with the foundational principles of episodic binding and retrieval holds promise for developing a comprehensive model capable of integrating binding effects across different levels of abstraction. A generalized binding mechanism, as represented by the binding matrix, can exert influence on task switching behavior on two different levels. Bindings that encompass visual or semantic features that cue the correct task rule can influence the task rule retrieval process. Simultaneously, this same mechanism can modulate the subsequent translation process, transforming stimulus input into motor output. Furthermore, hierarchical elements within task control are integrated through an updating process that diminishes the influence of older bindings when new task rules are implemented, thus circumventing the necessity for a hierarchical structure within the binding matrix itself.

4.3 Conclusion

This thesis addresses three key questions. First, it investigates whether the level of abstraction at which cognitive control operates influences its temporal dynamics and whether these findings can be transferred to control bindings. The results of two studies demonstrated the resilience of abstract control states and the bindings of abstract control states and visual contexts against varying temporal delays. These findings underscore the relevance of abstract information in learning processes across time. Second, the thesis examined whether a control retrieval process, as proposed by episodic binding and retrieval accounts, is the underlying mechanism of the C-CSE. In three experiments, no compelling evidence was found to support the idea of control state retrieval. Instead, the results suggest that the C-CSE primarily arises from attentional reset triggered by contextual changes. Finally, this thesis explored whether effects of abstract control states bound to visual contexts, as previously observed in the field of attentional control, could also be observed in task control. Across three experiments, evidence for such task rule bindings was observed, indicating a binding mechanism that generalizes across multiple facets of cognitive control.

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Appendix

A.1   Study I: Preregistered omnibus ANOVA approach for the CSE across RSI level analyses

We analyzed CSE scores across RSI levels using a repeated measures ANOVA with the within-participant factor RSI level [Experiment 1-3: 1000 ms vs 3000 ms vs 5000 ms; Experiment 4: 1000 ms vs 3000 ms vs 5000 ms vs 7000 ms vs 9000ms]. Experiment 2-4 analyzed additionally the within-participant factor task condition [Experiment 2 and 3: ´repetition´ design vs ´confound-minimized´ design; Experiment 4: 4-SR task complexity vs 6-SR task complexity]. Experiment 3 analyzed additionally the between-participant factor task condition order [repetition $\rightarrow$ confound-minimized vs confound-minimized $\rightarrow$ repetition]. The CSE was the dependent variable in all analyzes. Analyzes were conducted for RTs and error rates.

Experiment 1 - RTs. The main effect of the factor RSI level was not significant, $F(2, 98) = 0.03, p = .971$.

Experiment 1 - Error rates. The main effect of the factor RSI level was not significant, $F(2, 98) = 0.38, p = .683$.

Experiment 2 - RTs. The main effect of the factor RSI level was not significant, $F(2, 98) = 0.03, p = .971$. The main effect of the factor RSI level was not significant, $F(2, 88) = 1.06, p = .350$. The main effect of the factor task condition was not significant, $F(2, 44) = 0.99, p = .324$. The interaction between the two main effects was not significant, $F(2, 88) = 0.03, p = .971$.

Experiment 2 - Error rates. The main effect of the factor RSI level was not significant, $F(2, 88) = 0.82, p = .443$. The main effect of the factor task condition was significant, $F(1, 44) = 5.85, p = .020, \eta_p^2 = .117$, because participants showed a larger CSE in the ´repetition´ design ($M = 2.1\%$) than in ´confound-minimized´ design ($M = -1.1\%$). The interaction between the two main effects was not significant, $F(1, 88) = 1.24, p = .294$.

Experiment 3 - RTs. The main effect of the factor RSI level was not significant, $F(2, 94) = 1.36, p = .263$. The main effect of the factor task condition was not significant, $F(1, 47) = 0.03, p = .855$. The main effect of the factor

task order was not significant, $F(1, 47) = 2.74, p = .104$. The two-way interaction between the factors RSI level and task condition was not significant, $F(2, 94) = 2.88, p = .061$. The two-way interaction between the factors RSI level and task order was not significant, $F(2, 94) = 0.75, p = .475$. The two-way interaction between the factors task condition and task order was not significant, $F(1, 47) = 1.12, p = .296$. The three-way interaction including all factors was not significant, $F(2, 94) = 1.61, p = .205$.

Experiment 3 - Error rates.   The main effect of the factor RSI level was not significant, $F(1, 47) = 2.08, p = .130$. The main effect of the factor task condition was significant, $F(1, 47) = 4.82, p = .033, \eta_p^2 = .093$, because participants showed a larger CSE in the ´repetition´ design ($M = 3.7\%$) than in ´confound-minimized´ design ($M = -1.1\%$). The main effect of the factor task order was not significant, $F(1, 47) = 0.12, p = .734$. The two-way interaction between the factors RSI level and task condition was significant, $F(2, 94) = 3.66, p = .029, \eta_p^2 = .072$. Pairwise contrasts revealed a significant CSE difference between the 1000 ms ($M = 7.2\%$) and the 3000 ms RSI level ($M = 0.9\%$) in the ´repetition´ design, $t(188) = -3.308, p = .003$. The two-way interaction between the factors RSI level and task order was not significant, $F(2, 94) = 0.37, p = .691$. The two-way interaction between the factors task condition and task order was not significant, $F(1, 47) = 0.15, p = .704$. The three-way interaction including all factors was significant, $F(2, 94) = 3.32, p = .040, \eta_p^2 = .066$. Two follow up ANOVAS for the respective task order were conducted. For the confound-minimized→repetition condition a two-way interaction between the factors RSI level and task condition was observed, $F(2, 46) = 10.38, p < .001, \eta_p^2 = .311$. This effect was not found for the repetition→ confound-minimized task order condition.

Experiment 4 - RTs.   The main effect of the factor RSI level was not significant, $F(4, 264) = 0.11, p = .904$. The main effect of the factor task complexity was not significant, $F(1, 66) = 0.11, p = .740$. The interaction between the two main effects was significant, $F(4, 246) = 2.82, p = .025, \eta_p^2 = .041$. Pairwise contrasts revealed a significant CSE difference between ´4 S-R´ ($M = 35ms$) and the ´6 S-R´ complexity condition ($M = -1ms$) at the 3000 ms RSI level.

Experiment 4 - Error rates.   The main effect of the factor RSI level was significant, $F(4, 246) = 2.55, p = .040, \eta_p^2 = .037$. Pairwise contrasts revealed that participants showed a larger CSE in the 1000 ms RSI level ($M = 3.2\%$)

than in ´confound-minimized´ design ($M = -1.1\%$), $t(264) = -2.928, p = .030$. The main effect of the factor task complexity was not significant, $F(1, 66) = 0.91, p = .344$. The interaction between the two main effects was not significant, $F(4, 246) = 0.97, p = .427$.

A.2   Study II: Results of individual Bayesian analyses, separately for each Experiment and the originally preregistered analysis plan

Analyses of the individual Experiments

Before our main analysis, we wanted to validate whether the paradigm produced CSEs. For this purpose, we analyzed only context repetitions with short ITIs, a condition that mirrors previous research that has found robust CSEs. A Bayesian ANOVA was computed with the within factors current congruency [congruent vs incongruent] and previous congruency [congruent vs incongruent] and participant as random factor using mean RTs as dependent variables. In the mega-analysis, we additionally included the between-participant factor "experiment".

Experiment 1   The Bayesian ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed the following Bayes factors for main effects. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 5.540x1047(\pm1.31\%)$, because RTs were faster in congruent trials ($M = 563ms$) than in incongruent trials ($M = 723ms$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 3.376(\pm1.84\%)$. A CSE was represented by extreme evidence for the two-way interaction of current and previous congruency, $BF_{10} = 388.823(\pm52.47\%)$, because congruency effects were smaller after incongruent ($M = 136ms$) than after congruent trials ($M = 184ms$).

The same analysis on error rates revealed the following Bayes factors. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 129153.6(\pm0.79\%)$, because error rates were lower in congruent trials ($M = 5.6\%$) than in incongruent trials ($M = 9.4\%$). Second, there Bayes factors remained indecisive regarding the main effect of previous congruency. Additionally, Bayes factors for the two-way interaction between current and previous congruency remained indecisive.

Experiment 2   Bayesian ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed these Bayes factors for

main effects. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 2.854x1070(\pm1.31\%)$, because RTs were faster in congruent ($M = 555ms$) than in incongruent trials ($M = 720ms$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 5.900(\pm1.74\%)$. A CSE was represented by extreme evidence for the two-way interaction of current and previous congruency, $BF_{10} = 83096.68(\pm53.36\%)$, because congruency effects were smaller after incongruent ($M = 139ms$) than after congruent trials ($M = 190ms$).

The same analysis on error rates revealed the following Bayes factors. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 466548.6(\pm0.8\%)$, because error rates were lower in congruent ($M = 4.4\%$) than in incongruent trials ($M = 7.1\%$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 3.492(\pm1.02\%)$. Additionally, Bayes factors for the two-way interaction between current and previous congruency remained indecisive.

Experiment 3   Bayesian ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed these Bayes factors for main effects. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 1.261x1072(\pm1.32\%)$, because RTs were faster in congruent ($M = 587ms$) than in incongruent trials ($M = 754ms$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 6.363(\pm1.81\%)$. A CSE was represented by extreme evidence for the two-way interaction of current and previous congruency, $BF_{10} = 5013.562(\pm53.47\%)$, because congruency effects were smaller after incongruent ($M = 144ms$) than after congruent trials ($M = 190ms$).

The same analysis on error rates revealed the following Bayes factors. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 10018.18(\pm0.8\%)$, because error rates were lower in congruent ($M = 4.7\%$) than in incongruent trials ($M = 6.9\%$). Second, there was moderate evidence for a main effect of previous congruency, $BF_{10} = 5.603(\pm1\%)$, because error rates were lower in trials following congruent trials ($M = 5.1\%$) than in trials following incongruent trials ($M = 6.5\%$). Additionally, Bayes factors for the two-way interaction between current and previous congruency remained indecisive.

Experiment 4   Bayesian ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed these Bayes factors for main effects. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 1.553x1076(\pm1.32\%)$, because RTs were faster in congruent ($M = 540ms$) than in incongruent trials ($M = 670ms$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 6.111(\pm1.79\%)$. A CSE was represented by extreme evidence for the two-way interaction of current and previous congruency, $BF_{10} = 267.712(\pm53.35\%)$, because congruency effects were smaller after incongruent ($M = 135ms$) than after congruent trials ($M = 168ms$).

The same analysis on error rates revealed the following Bayes factors. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 1.299x1010(\pm0.8\%)$, because error rates were lower in congruent ($M = 4.2\%$) than in incongruent trials ($M = 8.0\%$). Second, Bayes factors remained indecisive in regard to a main effect of previous congruency. Finally, there was extreme evidence for the two-way interaction between current and previous congruency, $BF_{10} = 5.614x109(\pm9.83\%)$, because congruency effects were smaller after incongruent ($M = 3.1\%$) than after congruent trials ($M = 4.5\%$).

Experiment 5   Bayesian ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed the following Bayes factors for main effects. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 1.604x10109(\pm1.32\%)$, because RTs were faster in congruent ($M = 644ms$) than in incongruent trials ($M = 786ms$). Second, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 5.693(\pm1.96\%)$. A CSE was represented by extreme evidence for the two-way interaction of current and previous congruency, $BF_{10} = 154.036(\pm53.44\%)$, because congruency effects were smaller after incongruent ($M = 129ms$) than after congruent trials ($M = 156ms$).

The same analysis on error rates revealed the following Bayes factors. First, there was extreme evidence for a main effect of current congruency, $BF_{10} = 3.740x1016(\pm0.81\%)$, because error rates were lower in congruent ($M = 5.6\%$) than in incongruent trials ($M = 10.0\%$). Second, Bayes factors remained indecisive in regard to a main effect of previous congruency. Finally, Bayes factors for the two-way interaction between current and previous congruency remained indecisive.

Mega-analysis   The Bayesian ANOVA testing for CSEs in RTs in the context repetition condition with short ITIs revealed the following Bayes factors. First, the Bayes factor indicated extreme evidence for a main effect of experiment, $BF_{10} = 49651.74(\pm0.68\%)$. Pairwise Bayesian t-tests revealed decisive evidence for faster mean RTs in Experiment 4 ($M = 614ms$) compared to Experiment 2 ($M = 658ms$), $BF_{10} = 10.411(\pm0\%)$, and compared to Experiment 3 ($M = 669ms$), $BF_{10} = 35.379(\pm0\%)$. Additionally, we found decisive evidence for slower mean RTs in Experiment 5 ($M = 712ms$) compared to Experiment 1 ($M = 641ms$), $BF_{10} = 13.325(\pm0\%)$, compared to Experiment 2 ($M = 658ms$), $BF_{10} = 15.156(\pm0\%)$, and compared to Experiment 4 ($M = 614ms$), $BF_{10} = 16249.23(\pm0\%)$. Second, there was extreme evidence for a main effect of current congruency, $BF_{10} = 8.966x10380(\pm2.02\%)$, because RTs were faster in congruent ($M = 589ms$) than in incongruent trials ($M = 746ms$). Third, there was moderate evidence against a main effect of previous congruency, $BF_{01} = 3.747(\pm0.84\%)$. Most importantly, there was extreme evidence for the two-way interaction between current and previous congruency reflecting the CSE, $BF_{10} = 5.992x1016(\pm4.71\%)$, indicating that congruency effects were smaller after incongruent trials ($M = 137ms$) than after congruent trials ($M = 166ms$). There was strong evidence against a three-way interaction also including the experiment factor, $BF_{01} = 11.922(\pm12.96\%)$, indicating that the CSE effect did not differ between experiments.

The same analysis in error rates revealed these Bayes factors. First, Bayes factors for an effect of the factor experiment remained indecisive. Second, there was extreme evidence for a main effect of current congruency, $BF_{10} = 1.899x1045(\pm0.84\%)$, because error rates were lower in congruent trials ($M = 5.0\%$) than in incongruent trials ($M = 8.5\%$). Third, there was extreme evidence for a main effect of previous congruency, $BF_{10} = 894.473(\pm2.71\%)$, because error rates were lower in trials following incongruent trials ($M = 6.2\%$) than in trials following congruent trials ($M = 7.3\%$). There was strong evidence for the two-way interaction between current and previous congruency representing the CSE, $BF_{10} = 14.785(\pm13.47\%)$, indicating that congruency effects were smaller after incongruent ($M = 2.7\%$) than after congruent trials ($M = 4.1\%$). Finally, Bayes factors showed extreme evidence against a three-way interaction including the experiment factor $BF_{01} = 107.946(\pm7.47\%)$, indicating that the error CSE did not deviate from experiment to experiment.

Originally preregistered analysis plan

Experiment 1

According to our preregistration, we excluded the first trial of each block (0.8 %) and all trials following error trials (7.1 %). For RT analysis, we also removed all error trials (7.2 %) and trials deviating more than 3 SD from this subjects conditional mean RT (1.3 %). Mean RTs were calculated on an average of 55 observations (SD = 5.5) per condition (with 16 factorial cells: four congruency transitions, two context transitions, and two ITI conditions).

CSE validation.   The ANOVA testing for meaningful CSEs in RTs in the context repetition condition with short ITIs revealed two main effects. First, there was a main effect of current congruency, $F(1,44) = 3841.58, p < .001$, $\eta_p^2 = .872$, because RTs were faster in congruent ($M = 563ms$) than in incongruent trials ($M = 723ms$). Second, there was a main effect of previous congruency, $F(1,44) = 19.97, p < .001, \eta_p^2 = .312$, because RTs were faster in trials following congruent trials ($M = 635ms$) than in trials following incongruent trials ($M = 652ms$). A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1,44) = 79.96, p < .001, \eta_p^2 = .645$, because congruency effects were smaller after incongruent ($M = 136ms$) than after congruent trials ($M = 184ms$).

The same analysis on error rates revealed two main effects. First, there was a main effect of current congruency, $F(1,44) = 21.45, p < .001, \eta_p^2 = .328$, because error rates were lower in congruent ($M = 5.6\%$) than in incongruent trials ($M = 9.4\%$). Second, there was a main effect of previous congruency, $F(1,44) = 9.43, p = .004, \eta_p^2 = .176$, because error rates were lower in trials following incongruent trials ($M = 6.6\%$) than in trials following incongruent trials ($M = 8.4\%$). No other effect was significant ($p >= .293$).

Time course of the c-CSE.   The ANOVA testing our main hypothesis, the temporal decay of CSE context modulations for RTs, revealed no significant effects ($p >= .099$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1,44) < 1, p = .600$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition

$(BF_{01} = 5.424)$.

The same analysis on error rates revealed no significant effects as well $(p >= .107)$. The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 44) < 1, p = .467$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition $(BF_{01} = 4.805)$.

Experiment 2

The analysis plan for Experiment 2 was identical to Experiment 1. According to the preregistration, we excluded the first trial of each block (0.8 %) and all trials following error trials (5.6 %). For RT analysis we also removed all error trials (5.6 %) and trials deviating more than 3 SD from this subjects conditional mean RT (1.4 %). Mean RTs were calculated on an average of 56 observations $(SD = 5.0)$ per condition.

CSE validation. The ANOVA testing for meaningful CSEs in RTs in the context repetition condition with short ITIs revealed two main effects. First, there was a main effect of current congruency, $F(1, 59) = 518.11, p < .001, \eta_p^2 = .898$, because RTs were faster in congruent $(M = 555ms)$ than in incongruent trials $(M = 720ms)$. Second, there was a main effect of previous congruency, $F(1, 59) = 7.68, p = .007, \eta_p^2 = .115$, because RTs were faster in trials following congruent trials $(M = 655ms)$ than in trials following incongruent trials $(M = 642ms)$. A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1, 59) = 71.88, p < .001, \eta_p^2 = .549$, because congruency effects were smaller after incongruent $(M = 139ms)$ than after congruent trials $(M = 190ms)$.

The same analysis on error rates revealed a main effect of current congruency, $F(1, 59) = 24.04, p < .001, \eta_p^2 = .290$, because error rates were lower in congruent $(M = 4.4\%)$ than in incongruent trials $(M = 7.1\%)$. No other effect was significant $(p >= .155)$.

Time course of the c-CSE. The ANOVA testing our main hypothesis, the temporal decay of CSE context modulations for RTs, revealed a significant main effect of ITI duration, $F(1, 59) = 4.87, p = .031, \eta_p^2 = .076$, because CSEs were smaller in the long ITI condition $(M = 31.3ms)$ than in the short ITI

condition ($M = 46.6ms$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 59) < 1, p = .604$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 6.219$). The remaining effect of context transition was not significant ($p = .151$).

The same analysis on error rates revealed a significant main effect of context transition, $F(1, 59) = 4.60, p = .036, \eta_p^2 = .072$, since CSEs were larger in context repetition trials ($M = 1.4\%$) than in context change trials ($M = -0.1\%$). Further, we observed a two-way interaction between context transition and ITI duration, which tests our main hypothesis, $F(1, 59) = 4.90, p = .031, \eta_p^2 = .077$, indicating that the modulation of the CSE by context transitions was larger in blocks with long ITIs ($\Delta = 3.1\%$) than in blocks with short ITIs ($\Delta = 0\%$). Please note that this effect goes in the opposite direction as expected. The corresponding Bayes Factors remained undecisive. The remaining effect of ITI duration was not significant ($p >= .151$).

Experiment 3

The analysis plan for Experiment 3 was identical to the previous experiments. According to the preregistration, we excluded the first trial of each block (0.8 %) and all trials following error trials (5.9 %). For RT analysis we also removed all error trials (5.9 %) and trials deviating more than 3 SD from this subjects conditional mean RT (1.4 %). Mean RTs were calculated on an average of 56 observations ($SD = 4.9$) per condition.

CSE validation. The ANOVA testing for meaningful CSEs in RTs in the context repetition condition with short ITIs revealed two main effects. First, there was a main effect of current congruency, $F(1, 59) = 503.59, p < .001, \eta_p^2 = .895$, because RTs were faster in congruent ($M = 587ms$) than in incongruent trials ($M = 754ms$). Second, there was a main effect of previous congruency, $F(1, 59) = 6.09, p = .017, \eta_p^2 = .094$, because RTs were faster in trials following congruent trials ($M = 667ms$) than in trials following incongruent trials ($M = 674ms$). A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1, 59) = 56.59, p < .001, \eta_p^2 = .490$, because congruency effects were smaller after incongruent ($M = 144ms$) than

after congruent trials ($M = 190ms$).

The same analysis on error rates revealed two main effects. First, there was a main effect of current congruency, $F(1, 59) = 17.86, p < .001, \eta_p^2 = .232$, because error rates were lower in congruent ($M = 4.7\%$) than in incongruent trials ($M = 6.9\%$). Second, there was a main effect of previous congruency, $F(1, 59) = 13.35, p < .001, \eta_p^2 = .184$, because error rates were lower in trials following incongruent trials ($M = 5.1\%$) than in trials following incongruent trials ($M = 6.5\%$). A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1, 59) = 7.03, p = .010, \eta_p^2 = .106$, because congruency effects were smaller after incongruent ($M = 1.2\%$) than after congruent trials ($M = 3.2\%$).

Time course of the c-CSE. The ANOVA testing our main hypothesis, the temporal decay of CSE context modulations for RTs, revealed no significant effects ($p >= .161$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 59) < 1, p = .161$. Bayes Factors for the test of the main hypothesis indicated anecdotal evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 2.741$).

The same analysis on error rates revealed a significant main effect of ITI duration, $F(1, 59) = 8.34, p = .005, \eta_p^2 = .124$, since CSEs were larger in blocks with smaller ITIs ($M = 0.0\%$) than in blocks with short it is ($M = 1.9\%$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 59) < 1, p = .845$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 6.951$). The remaining effect of context transition was not significant ($p = .428$).

Experiment 4

The analysis plan for Experiment 4 was identical to the previous experiments. According to the preregistration, we excluded the first trial of each block (0.8 %) and all trials following error trials (5.8 %). For RT analysis we also removed all error trials (5.7 %) and trials deviating more than 3 SD from

this subjects conditional mean RT (1.4 %). Mean RTs were calculated on an average of 56 observations (SD = 5.9) per condition.

CSE validation. The ANOVA testing for meaningful CSEs in RTs in the context repetition condition with short ITIs revealed revealed two main effects. First, there was a main effect of current congruency, $F(1, 60) = 515.18, p < .001, \eta_p^2 = .896$, because RTs were faster in congruent ($M = 540ms$) than in incongruent trials ($M = 670ms$). Second, there was a main effect of previous congruency, $F(1, 60) = 8.44, p = .005, \eta_p^2 = .123$, because RTs were faster in trials following congruent trials ($M = 613ms$) than in trials following incongruent trials ($M = 620ms$). A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1, 59) = 56.73, p < .001, \eta_p^2 = .486$, because congruency effects were smaller after incongruent ($M = 135ms$) than after congruent trials ($M = 168ms$).

The same analysis on error rates revealed two main effects. First, there was a main effect of current congruency, $F(1, 59) = 34.65, p < .001, \eta_p^2 = .366$, because error rates were lower in congruent ($M = 4.2\%$) than in incongruent trials ($M = 8.0\%$). Second, there was a main effect of previous congruency, $F(1, 59) = 7.25, p = .009, \eta_p^2 = .176$, because error rates were lower in trials following incongruent trials ($M = 5.6\%$) than in trials following incongruent trials ($M = 6.5\%$). No other effect was significant ($p >= .081$).

Time course of the c-CSE. The ANOVA testing our main hypothesis, the temporal decay of CSE context modulations for RTs, revealed a significant main effect of context transition, $F(1, 60) = 4.20, p = .045, \eta_p^2 = .065$, since CSEs were larger in context repetition trials ($M = 32.3ms$) than in context change trials ($M = 23.4ms$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 60) < 1, p = .954$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 7.124$). The remaining effect of ITI duration was not significant ($p = .828$).

The same analysis on error rates revealed no significant effects ($p >= .225$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 60) < 1, p > .999$.

Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 7.136$).

Experiment 5

The analysis plan for Experiment 5 was identical to the previous experiments. According to the preregistration, we excluded the first trial of each block (0.4 %) and all trials following error trials (7.8 %). For RT analysis we also removed all error trials (7.8 %) and trials deviating more than 3 SD from this subjects conditional mean RT (1.1 %). Mean RTs were calculated on an average of 55 observations (SD = 7.8) per condition.

CSE validation. The ANOVA testing for meaningful CSEs in RTs in the context transition condition with short ITIs revealed the following main effects. First, there was a main effect of current congruency, $F(1, 100) = 663.70, p < .001, \eta_p^2 = .869$, because RTs were faster in congruent ($M = 644ms$) than in incongruent trials ($M = 786ms$). Second, there was a main effect of previous congruency, $F(1, 100) = 15.21, p < .001, \eta_p^2 = .132$, because RTs were faster in trials following congruent trials ($M = 709ms$) than in trials following incongruent trials ($M = 718ms$). A significant CSE was represented by the two-way interaction of current and previous congruency, $F(1, 100) = 34.39, p < .001, \eta_p^2 = .256$, because congruency effects were smaller after incongruent ($M = 129ms$) than after congruent trials ($M = 156ms$).

The same analysis on error rates revealed two main effects. First, there was a main effect of current congruency, $F(1, 100) = 53.65, p < .001, \eta_p^2 = .349$, because error rates were lower in congruent ($M = 5.6\%$) than in incongruent trials ($M = 10.0\%$). Second, there was a main effect of previous congruency, $F(1, 100) = 8.44, p = .005, \eta_p^2 = .078$, because error rates were lower in trials following incongruent trials ($M = 8.2\%$) than in trials following incongruent trials ($M = 9.4\%$). No other effect was significant ($p >= .081$). Finally, there was a significant two-way interaction of current and previous congruency, $F(1, 100) = 4.16 p = .044, \eta_p^2 = .040$.

Time course of the c-CSE. The ANOVA testing our main hypothesis, the temporal decay of CSE context modulations for RTs, revealed a significant main effect of context transition, $F(1, 100) = 6.28, p = .014, \eta_p^2 = .060$, since CSEs were larger in context repetition trials ($M = 36.3ms$) than in context change

trials ($M = 24.1 ms$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 100) < 1, p = .505$. Bayes Factors for an undirected test of the main hypothesis remained undecisive, but indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 22.551$), when using a directed which seems reasonable since surprisingly the c-CSE became descriptively larger in the long ITI condition. The remaining effect of ITI duration was not significant ($p = .422$).

The same analysis on error rates revealed no significant effects ($p >= .422$). The two-way interaction between context transition and ITI duration representing the test of our main hypothesis was not significant, $F(1, 100) < 1, p > .505$. Bayes Factors for the test of the main hypothesis indicated moderate evidence in favor of the null model, i.e. no difference in the context modulation of the CSE between the short and long ITI condition ($BF_{01} = 2.584$).